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THE

AMERICAN JOURNAL OF SCIENCE

[FOURTH SERIES.]

ART. XLII.—*Studies of Eocene Mammalia in the Marsh Collection, Peabody Museum;* by J. L. WORTMAN.

[Continued from p. 414.]

Origin of the Primates.

IN any attempt at a consideration of the question of the origin of the Primates, we are met almost at the very threshold of our inquiry by such a lack of definite information as effectually to bar our progress, in so far, at least, as positive or decided results are concerned. The only course that is open to us at present is to reason by induction and analogy, but it may well be that this method will prove futile enough and furnish a very unsafe guide in threading our way across this unexplored waste in the knowledge of simian history.

The first question of importance to discuss in this connection is the probable location of the place of origin of the Primates, and just here some welcome facts come to our aid. From the abundant and well-attested records of paleobotany, we learn first of all that a tropical vegetation flourished within the Arctic Circle as late as the beginning of the Cretaceous, and for reasons which will be presently given it appears highly probable that this was one of the regions in which climatic environment first presented sufficiently favorable conditions for the beginnings of higher forms of plant life. In other words, it was probably the original home of the Angiosperms, or flowering plants. The existence of the higher types of Mammalia was manifestly impossible before the appearance of the necessary plants upon which they so largely depend for food, and I shall therefore assume the existence of a close and intimate relationship between the development of the one and the origin of the other.

Just what climatic conditions obtained in the polar regions prior to the Cretaceous, whether there were alternate periods of heat and cold with consequent glaciation, or whether the climate was uniformly tropical, are questions with which our present inquiry is not vitally concerned, for the reason that the origin of the higher forms of mammalian life can not be consistently placed at a much earlier date than the beginning of the Cretaceous.

Touching the question of former glacial epochs, however, it is proper to state that more recent geological investigations have shown the existence of boulders of great size and thickness, scratched and glacial-like, in beds of Permian and probably of Triassic age also, in Australia, New Zealand, South Africa, India, and presumably in South America. The wide-spread occurrence of these phenomena over the Southern Hemisphere would seem to be impossible of explanation on any hypothesis other than that of glaciation or a period of cold. Such facts have led many geologists to believe in the existence of successive or intermittent glacial epochs in the past. A fair estimate of present opinion on the subject may be gained from Dana's statement,* which is as follows: "Thus, throughout the earth's history since life began, the only cold epochs of which proof has been found occurred near or at the close of the Permian, at the close of the Triassic, and during the Glacial period. At the close of the Cretaceous, another epoch is suspected to have occurred, but without direct evidence."

With a few exceptions the evidence of glaciation in the Northern Hemisphere is confined almost exclusively to the Glacial epoch proper, and it is to be seriously doubted whether conclusive proof of wide-spread glacial action in any former period will ever be found. At all events we are not warranted in the belief that a frigid temperature overspread the Northern Hemisphere at any time between the Jurassic and the Tertiary. On the contrary, all the testimony which has yet been gathered from various available sources shows that there were slow and gradual changes of temperature from tropical, through all the stages of subtropical, warm temperate, temperate, cold temperate, subfrigid, and frigid, in the long interval between the Jurassic and the Glacial epochs.

The class of facts upon which we most largely depend for conclusions respecting the former climatic conditions in any region are furnished by the fossil remains of plants and animals imbedded in its strata. In order that evidence of this character may become available, we must assume two propositions: (1)

* Manual of Geology, 1894, p. 1027.

That the animals and plants lived and died in the vicinity where their fossil remains occur; and (2) that by comparison with their nearest living allies we may deduce some information respecting their habits. For example, we know that such plants as Palms, Cycads, and Tree-Ferns require a given mean temperature for their existence. Frost quickly destroys them and they rapidly disappear where much freezing takes place. Their northernmost limit is found, therefore, at or near the isothermal line separating a warm temperate from a subtropical temperature. Thus, while a few of the hardier forms may be distributed along the borders, the vast majority of the species inhabit the more central tropical and subtropical parts of the earth, where they form a conspicuous and characteristic feature of the flora.

If, therefore, the remains of such forms as Palms, Cycads, or Tree-Ferns occur as fossils in a given locality, we may conclude without fear of error that the temperature of such place could not have been below that which at present limits their distribution, yet it may at the same time have been tropical or subtropical. Some information regarding the latter condition may be had from the relative abundance of remains of such species. If very abundant and in considerable variety, a subtropical or tropical temperature rather than that bordering on a warm temperature would be indicated. In the same way, we may assume the presence of remains of certain species of extinct animals to be indicative of, and equally conclusive proof of, temperatures in which their nearest allies live at the present day.

With this understanding of the nature of the evidence upon which we must rely, I pass next to a brief review of the former distribution of the plants in the Northern Hemisphere, and shall begin with a consideration of the fossil floræ of the Arctic regions. Certain deposits in Greenland, near Disco Island, 70° 33′ N. Lat.—the Komé beds—have yielded a rich flora, which according to Heer, its describer, is strictly comparable with that of the Neocomian or Lower Cretaceous of Europe and America. It includes Ferns, Cycads, Conifers, and a single species of Angiosperm—a Poplar. Of the Ferns, the genus *Gleichenia*, now almost exclusively tropical in its distribution, is represented by fifteen species. There are also four genera of Cycads, with numerous species, whose nearest living representatives now inhabit the tropical and subtropical regions. Pines, Redwoods, and other Conifers, some closely related species to which are now found living in China and California, go to make up the list of the Gymnosperms. The Angiosperms, as far as we now know, are represented by the single genus and

species *Populus primæva*, which is probably the oldest Angiosperm thus far discovered.

In such distant localities within the Polar regions as Spitzbergen and Alaska are found deposits of nearly or quite the same age, and here the same or a closely allied flora occurs. The conclusion is therefore obvious that the climatic conditions by which this plant life was surrounded were not confined to a single locality and were not the result of local changes, but this evidence compels us to believe that they were wide-spread and general over the whole Arctic region during this period. The mean temperature indicated by these plants, according to Heer,* than whom no better authority could be quoted, was from 21° to 22° C., or 70° to 72° F. This brings it very near the isothermal line 74° F., which separates the subtropical from the tropical temperature; hence the climate may be said to have been virtually tropical.

Deposits corresponding closely in age to that of the Upper Cretaceous of Europe and North America are found in the same latitude, near the same locality as that just described. These beds—the Atané—have furnished the remains of a flora no less remarkable than that of the Lower Cretaceous. In all, sixty-five species have been identified, of which fifteen are Ferns, two Cycads, eleven Conifers, three Monocotyledons, and thirty-four Dicotyledons. Of the Ferns, one is a Tree-Fern, which, with the Cycads and other species, at once gives a subtropical aspect to the flora. The Dicotyledons include such types as the Poplars, Bayberry and Sweet Fern, Fig, Sassafras, Heath, Cinnamon, Persimmon, Aralia or Ginseng, Magnolia, Myrtle, and Legumes. The living representatives of many of these species are now found in a subtropical or warm temperate climate, and if such evidence has any value it furnishes incontestable proof that the temperature of the Arctic regions, although still tropical, had suffered a decline from that of the Lower Cretaceous during the latter part of the period.

Turning next to the Tertiary, we find from the same class of evidence that wide-spread changes in the climatic conditions had occurred. There are deposits at many localities within the Arctic regions in which plant remains of this epoch are found, but their exact equivalents among European and American beds are difficult of determination. They were referred by Heer to the Miocene, but Dawson expressed the opinion that they were at least in part Eocene in age—a view which is more likely the correct one. Further exploration and the discovery of animal remains are necessary before these matters can be finally cleared up. At all events, it is perfectly

* See Professor Heer's famous work, Flora Fossilis Arctica.

certain that they are later in age than those containing the Upper Cretaceous flora above described.

As Wallace gives such an excellent summary of what is known on this subject, I quote his statement in full. He says:* "One of the most startling and important of the scientific discoveries of the last forty years has been that of the relics of a luxuriant flora in the various parts of the Arctic regions. It is a discovery that was totally unexpected and is even now considered by many men of science to be completely unintelligible; but it is so thoroughly established, and it has such a direct and important bearing on the subjects we are discussing in the present volume, that it is necessary to lay a tolerably complete outline of the facts before our readers.

"The Miocene flora of temperate Europe was very like that of Eastern Asia, Japan, and the warmer part of Eastern North America of the present day. It is very richly represented in Switzerland by well preserved fossil remains, and after a close comparison with the flora of other countries Professor Heer concludes that the Swiss Lower Miocene flora indicates a climate corresponding to that of Louisiana, North Africa, and South China, while the Upper Miocene climate of the same country would correspond to that of the south of Spain, Southern Japan, and Georgia (U. S. of America). Of this latter flora, found chiefly at Œningen in the northern extremity of Switzerland, 465 species are known, of which 166 species are trees or shrubs, half of them being evergreens. They comprise sequoias like the Californian giant trees, camphor-trees, cinnamons, sassafras, bignonias, cassias, gleditschias, tulip-trees, and many other American genera, together with maples, ashes, planes, oaks, poplars, and other familiar European trees represented by a variety of extinct species. If now we go to the west coast of Greenland in 70° N. Lat. we find abundant remains of a flora of the same general type as that of Œningen but of a more northern character. We have a sequoia identical with one of the species found at Œningen, a chestnut, salisburia, liquidambar, sassafras, and even a magnolia. We have also seven species of oaks, two planes, two vines, three beeches, four poplars, two willows, a walnut, a plum, and several shrubs supposed to be evergreens; altogether 137 species, mostly well and abundantly preserved!

"But even further north in Spitzbergen, in 78° and 79° N. Lat. and one of the most barren and inhospitable regions on the globe, an almost equally rich fossil flora has been discovered including several of the Greenland species, and others peculiar, but mostly of the same genera. There seem to be no evergreens here except coniferæ, one of which is identical with the

*Island Life, 1892, p. 183.

swamp-cypress *(Taxodium distichum)* now found living in the Southern United States! There are also eleven pines, two Libocedrus, two sequoias, with oaks, poplars, birches, planes, limes, a hazel, an ash, and a walnut; also water-lilies, pondweeds and an iris—altogether about a hundred species of flowering plants. Even in Grinnell Land, within $8\frac{1}{4}$ degrees of the pole, a similar flora existed, twenty-five species of fossil plants having been collected by the last Arctic expedition, of which eighteen are identical with the species from other Arctic localities. This flora comprised poplars, birches, hazels, elms, viburnums, and eight species of conifers, including the swamp cypress and the Norway spruce *(Pinus abies)*, which last does not now extend beyond $69\frac{1}{2}°$ N.

"Fossil plants closely resembling those just mentioned have been found at many other Arctic localities, especially in Iceland, on the Mackenzie River in 65° N. Lat. and in Alaska."

Thus it will be seen that proof in favor of the view that the Arctic regions enjoyed a mild and equable climate unbroken by periods of cold, up to and including the middle of the Tertiary, is simply incontestable and overwhelming. At the same time it is equally evident that while capable of supporting a luxuriant vegetation up to this period, the temperature, as indicated by the fossil plants, shows unmistakable signs of a slow and steady decline from a tropical to a temperate condition.

Let us next examine the evidence afforded by the plants as indicative of former climatic conditions in the regions now embraced within the North Temperate zone. Roughly speaking, we now transfer our attention some two thousand miles to the southward of these more typical localities in Greenland which have furnished the remains of the remarkable flora here discussed. Owing to more favorable circumstances and better facilities, the deposits in these latitudes have been much more extensively and thoroughly examined and their fossil contents more carefully collected and studied, than those of the Arctic regions. Moreover, we have in these deposits the remains of large numbers of extinct animals often associated with those of the plants, so that we are enabled to get a somewhat clearer idea of its ancient physical conditions. But in attempting to correlate the results of investigation obtained in one region with those in another, we have constantly to bear in mind the difficulties which beset the problem of establishing equivalency in the time scale in deposits of widely separated localities. We are accustomed to depend very largely, if not solely, upon the fossil contents of any two given strata for our ideas respecting their equivalency in age. It has no doubt often happened, however, that certain types of plants or animals originating in

one region were gradually distributed to others by migration, so that the deposits containing them would show the same or closely allied species in practically the same stages of development; yet this similarity in the contained fossils may not of necessity imply absolute synchronism of deposit, since a certain length of time must have been required for the migration, which would have been longer or shorter in proportion as the distance was great or small and the obstacles to be overcome were easy or difficult. It is just such complications as these that arise when we compare the deposits of the Arctic with those of the Temperate latitudes, and prevent a final and absolute determination of their exact equivalency.

In the North Temperate regions, the Mesozoic strata reach a high degree of development and in many places contain rich deposits of fossils. In Europe, the Triassic and Jurassic are much better developed than in America, while on the other hand the Cretaceous series is much more varied and extensive in America than in Europe.

The Jurassic of Europe has furnished a flora consisting of Conifers, Cycads, Ferns, Equiseta, and two Monocotyledons. In all, there are some one hundred and fifty species, of which over eighty are Ferns, twenty are Conifers, and thirty or forty are Cycads. The Conifers are represented by genera and species closely allied to the Yew, Pine, Arbor-Vitæ, Cypress, and Norfolk Island Pine. The two Monocotyledons are said to belong to the Arum and Pandanus families, or such types in our living flora as the Calla, Skunk Cabbage, and the Screw Pine. There are no Dicotyledons or higher Angiosperms known with certainty.

In America, the flora of the Jurassic is not so well represented, but the known species have a close relationship with those of the Jurassic of Europe. Thus, Conifers, Cycads, and Ferns are the only forms yet brought to light. The American Jura has hitherto furnished no Monocotyledons, and as in Europe, the Dicotyledons are completely absent. According to all authorities, the climatic conditions indicated by this flora, as well as by the associated animals, both Invertebrates and Vertebrates, are such as are now found in the vicinity of the Equator.

The Lower Cretaceous of Europe is well developed and in many places is capable of division into a number of substages. In England, the deposits belonging in this series are the Wealden, Lower Greensand or Aptian, and the Gault or Albian. In Portugal, at least six divisions are recognized, which correspond almost exactly with the subdivisions of the Potomac formation on the Atlantic border in this country. The flora of the Wealden, according to Ward, contains eighty species,

which include two Algæ, two Fucoids, one Hepatic, one Chara, three Equiseta, twenty-three Ferns, twenty-one Cycads, twenty-four Conifers, and one Monocotyledon. It is remarkable for the entire absence of Dicotyledons. The lowermost Cretaceous beds of Portugal, which are considered to be equivalent in age to the Wealden, have, according to the same authority, yielded two Algæ, two Hepatics, three Lycopods, two Equiseta, seventy-nine Ferns, fifteen Cycads, twenty-nine Conifers, sixteen Monocotyledons, forty-seven Dicotyledons, five anomalous types classified as Proangiosperms, and three forms of uncertain reference. In like manner, the Potomac formation of America contains Ferns, Cycads, Conifers, Monocotyledons, and Dicotyledons, in nearly the same proportion as the beds of Portugal. The character of this flora gives unmistakable evidence of a tropical climate, and while we are not able to say whether the temperature was actually higher than in Greenland during the deposition of the beds supposed to be of the same age, we may feel perfectly certain that tropical conditions were wide-spread over the Northern Hemisphere during the early part of the Cretaceous.

In the basal strata of the Lower Cretaceous, no remains of true Dicotyledons have as yet been found. Higher up in the series they occur sparingly, but toward the top of the section they suddenly appear in great numbers and variety. Thus, in the Gault of England and in beds of corresponding age in Portugal are found the leaves of Poplar, Magnolia, Myrtle, Willow, Walnut, Maple, Sassafras, Fig, Cinnamon, Holly, Oak, Redwood, and Palms. In America, in beds of approximately, if not identically, the same age, occur Sassafras, Tulip-Tree, Magnolia, Aralia, Cinnamon, Poplar, Willow, Maple, Birch, Chestnut, Alder, Beech, Elm, Sequoias, and Palms, the leaves of some of the latter being ten feet in diameter. The leaves of a few Cycads are present, but they are not so abundant as they were in the lower stages, and here represent a waning group.

In a survey of this flora as a whole, together with a consideration of the manner of occurrence of certain of its constituent elements, two facts of more than ordinary importance force themselves upon the attention of the investigator: (1) There is an astonishing similarity or striking resemblance between these Cretaceous plants of Europe and America; and (2) the higher Angiosperms appear in identically the same manner in these two widely separated areas, by sudden introduction as if by impulses or waves of migration.

In regard to the first of these propositions, Ward in his excellent paper has shown that the early Cretaceous floræ of

the two continents are very similar. He says,* "We should not, of course, expect the species to be common to any great extent, and the comparison is practically limited to the genera. Looked at from this point of view, we see that the resemblance is indeed close, a great number of the important genera occurring in both floras. There are no less than 46 of these common to the two, though in some cases the author's individuality is probably alone responsible for slight differences in the terminations in the names." What is here said of the plant remains from the lower beds is also true of those from the Cenomanian, and in fact the entire Cretaceous flora of the two Hemispheres is strikingly similar.

The second fact of unusual importance is the sudden introduction of the higher Angiosperms in the several strata in which they occur. They first make their appearance in beds below the Cenomanian, but with very few exceptions these species are all strictly representatives of, and belong to, the higher forms. They exhibit few traces of a lower organization, and certainly fall far short of the connecting forms we should expect to find bridging the gap between the Dicotyledons and lower types, if their evolution had taken place in the regions where their remains are now found. It is possible to explain the sudden influx of so extensive, varied, and highly organized a flora as that of the Cenomanian only on the hypothesis of migration, and this migration was, moreover, in all probability due to a climatic change which permitted the species to spread into new localities formerly unfitted for their maintenance. There is no other way in which to account for the remarkable similarity between the floræ of the two Hemispheres, it seems to me, than to assume the existence of a common center of dispersion. All the facts in connection with the distribution of these higher plants offer such a striking analogy to a similar class of facts concerning the higher Mammalia, that I shall have occasion to recur to this subject again.

In the Upper Cretaceous beds of both Europe and America, the remains of a rich flora have been found. I can not better illustrate the character of this flora in America than to quote a few extracts from the work of Professor L. F. Ward, one of the most eminent authorities on paleobotany in America. In describing the types of the Laramie flora, he says,† in speaking of the genus *Ficus:* "Among my Fort Union specimens I have thus far found three species referable to that genus which, if this reference is sustained, and if no others be detected, will show that a climate existed in the Fort Union epoch and at

* Some Analogies in the Lower Cretaceous of Europe and America. Sixteenth Ann. Rept. U. S. Geol. Surv., p. 534, 1895.

† Bull. U. S. Geol. Surv., No. 37, 1887.

the latitude of Glendive [Montana] warm enough and moist enough to permit these chiefly tropical plants to thrive." Again, in speaking of the genus *Cinnamomum*, he says: "An almost exclusively tropical genus embracing about fifty species, confined to the Old World, but ranging on both sides of the equator. Fossil representatives are abundant in the Tertiaries of Europe, especially in the Eocene, but forms are reported as low as the Cenomanian. The four species of the Laramie thus far described argue a warm climate." And again, he says of *Zizyphus*, a genus related to the Buckthorn: "A widely distributed tropical and subtropical genus containing in the present flora about fifty species. It has been hitherto represented by eight Eocene, one Green River and five Laramie species, none of the last being from the Fort Union strata." He describes three species from the Upper Cretaceous of Montana. In like manner he mentions a number of other types of a tropical or subtropical habit from the Laramie, or Upper Cretaceous beds. Lastly, I may mention the presence of Palms of the genus *Sabal*, recently described by Hatcher as an absolutely conclusive piece of evidence in favor of a Floridian climate during this epoch, as far north as Wyoming and Montana.

In the Tertiary, a subtropical climate continued in America as far north as central Wyoming, to near the close of the Eocene. This is proved by abundant remains of Palms in the Green River shale beds, and by the presence of numerous tropical species of Vertebrates and Invertebrates in the Bridger beds. Before the close of the Eocene, however, the temperature in this latitude underwent a change, and by the time the Oligocene is reached a strictly temperate climate had appeared. In Europe, similar changes in the climatic conditions throughout the Tertiary, from an almost, if not quite, tropical temperature through all the intermediate stages to that of a frigid temperature in Glacial time, are to be met with.

Viewed from the standpoint of the fossil plants, therefore, the contention that the Northern Hemisphere has passed through all these phases of climate from torrid to frigid, from early in the Mesozoic to the present, is simply incontestable. The evidence is equally conclusive that the cooling-off process was inaugurated at the Pole and gradually extended to the southward. The assumption, moreover, that the higher Angiosperms originated and were evolved in the more southern latitudes, where they so suddenly appear, is illogical and untenable, not only by reason of the virtual absence of any ancestral forms foreshadowing or leading up to them in the floræ of the older epochs, but by the sudden appearance of the same or closely allied types in identically the same manner and, as far as we

know, at the same time, in localities so widely separated as Europe and America.

If, now, we examine these problems from the standpoint of the distribution of the ancient mammalian faunæ in the Northern Hemisphere, we reach conclusions so strikingly similar, that the two classes of facts taken together constitute such a basis or groundwork of probability, that we may feel reasonably safe in locating the place of origin of both the higher plants and certain mammals, at least, within the polar regions.*

The facts of mammalian distribution in the Northern Hemisphere may be briefly stated as follows: Early in the Mesozoic, there appeared small mammal-like forms, which were widely distributed over both the Northern and Southern Hemispheres. Representatives of these species continued throughout the Cretaceous, and finally disappeared in the early stages of the Tertiary. From these forms the modern Marsupials undoubtedly had their origin. Many of them are classified in the group Multituberculata, which without much doubt finds its nearest living representative in the Duckbill of Australia. It should be here stated that with very few exceptions all these forms are represented by fragments of jaws and teeth. In one instance, however, a fairly complete skull is known (*Tritylodon*) from the Karoo beds of South Africa. The teeth of this species are astonishingly like those of many types in the Northern Hemisphere, and hitherto it has always been classified in this group. Seeley has shown that the organization of the skull presents so many reptilian characters as to cause him to refer it to the Reptilia. If this reference is correct, then in the absence of any facts to the contrary it is highly probable that all the Multituberculates are as much reptile as mammal. Indeed, it is not easy to say at first glance upon which side of the line living Monotremes should be placed. There can be little doubt that, when more fully known, these ancient fossil types will present every conceivable gradation between these two great divisions of the Vertebrata.

One fact in connection with these Mesozoic forms stands out clearly and distinctly, and that is that as far as we are permitted to judge from their fragmentary remains, the progress of their evolution toward any of the higher mammals was very slow indeed. The amount of change registered by their teeth throughout the greater part of the Mesozoic is comparatively insignificant. On this account it is inconceivable that any of the species with which we are at present acquainted should be regarded in the light of ancestors of the higher types, or Eutherian Mammalia.

* Some very noted botanists have held this view of the origin of the Angiosperms within the polar regions; thus Gray, Saporta, and Nathorst were convinced of its truth, and Dawson thought it probable.

The oldest Tertiary strata containing mammalian remains which are at present known are found in the San Juan Basin of New Mexico, and were named by Cope the Puerco. These beds are from five hundred to eight hundred feet in thickness, and apparently lie conformably upon the Laramie, or Upper Cretaceous. Within the lower fifty feet are found the remains of a rich mammalian fauna, thirty-one species in all, composed largely of representatives of the higher, or Eutherian, subclass. Associated with them are five species of Mesozoic types closely related to forms from the older Laramie beds.

In this same region, at a distance of from five to eight hundred feet above the Puerco layer, occurs a second fossil-bearing horizon, which I have called the Torrejon stage. From this bed, forty-four species have been identified, of which five are of the Mesozoic type. The facies of the fauna is very like that of the Puerco, and there are many genera or their derivatives common to the two horizons, but the species are in every case different. Just as the Eutherian mammals in the Puerco bed below were all of sudden introduction, so there were no less than twelve genera which represent entirely new and previously unknown types.

Resting conformably upon the Torrejon comes the Wasatch, the beginning of what we may call the middle division of the Eocene, and here again is a rich mammalian fauna. Deposits of this age are found in the Big Horn, Green River, and Wind River Basins of Wyoming and all have yielded many remains of extinct mammals. Like that of the preceding bed, the fauna of the Wasatch is made up of two elements: One which includes the modified descendants of the older preëxisting types, and another which is entirely new to the region. This latter element in the Wasatch comprises not only new genera and species, but entire new orders. Thus, we meet for the first time with such types as the Rodentia, Artiodactyla, Perissodactyla, besides whole suborders and families included in the Carnivora, Amblypoda, Primates, and Insectivora. Some of these types continue through several subsequent stages and then disappear, while others become the dominant factors in the succeeding development and finally give rise to the modern mammalian fauna. What is here said of the Puerco, Torrejon, and Wasatch, is likewise true of the Wind River, Bridger, and Uinta of the Eocene. New and strange forms constantly appear, of which no vestige nor trace has ever been found in the older beds. Nor is this sudden introduction of new types confined to the Eocene, but is equally true of the Oligocene, Miocene, and Pliocene beds of this country.

The most remarkable circumstance connected with the facts here enumerated is that identically the same thing happened in

the Tertiary deposits of Europe. The succession of these strata is not so complete in Europe as in America, and their record of the appearance and disappearance of mammalian forms is less perfect; however, it is sufficiently complete to give the broad features of mammalian succession. While the Jurassic mammals are well represented, and correspond closely genus for genus with those from the Wyoming beds in America, the Cretaceous forms as well as those corresponding to our Puerco have not been found in Europe. The oldest Tertiary beds in Europe from which mammalian remains have been obtained are those at Cernay in France. According to Osborn,* this fauna bears a close resemblance and corresponds to that of our Torrejon. It is in the succeeding Wasatch or its near equivalents, however, that the most striking similarity occurs. Exposures of beds of this age are found at many localities in Europe, and a fauna almost equal in richness and variety to that of America is known. Just as in America, the sudden introduction of the same entire orders—the true Primates, the Rodentia, Artiodactyla, Perissodactyla, Insectivora, as well as many suborders, families, and genera of the various other groups—takes place without previous warning. The most remarkable fact is the similarity observable in the two faunæ. In many instances, the species belong to the same genera which are common to the two Hemispheres.† In the following succession, the introduction of new types of the same general character as those in America proceeds in the same sudden and unheralded manner.

Now, what is the significance of these facts and from what source or sources were these faunæ derived? In the case of the Puerco, we have the sudden introduction of thirty or more species belonging mostly to a group of mammals hitherto entirely unknown upon the earth. The species of the Mesozoic types may be accounted for by derivation from the preëxisting mammals whose remains are found in the underlying Cretaceous, but as regards the Eutherian element of the fauna, no earlier traces of it have ever been found, and he who would maintain that this new Eutherian element in the Puerco fauna developed from any of the known Cretaceous forms, in the ordinary course of evolution, would be so utterly lacking in a proper sense of morphological proportion as to be a very unsafe guide in such matters. The Laramie or Upper Creta-

* A Review of the Cernaysian Mammalia, Proc. Phila. Acad., 1890.

† The more important of the nearly related or identical genera common to Europe and America in the Wasatch or its near equivalent in Europe, thus far identified, are *Coryphodon*, *Phenacodus*, *Hyracotherium*, *Pantolestes?*, *Paramys* and *Plesiarctomys*, *Heptodon* and *Lophiodon*, *Arctocyon* and *Anacodon*, *Palæonictis*, *Sinopa* and *Proviverra*, *Oxyæna*, *Calamodon*, *Hyopsodus*, and others.

ceous beds are extensively exposed in the same region in which the Puerco fossils are found, and indeed the remains of many characteristic Laramie land Vertebrates occur in the strata immediately underlying those containing the Puerco, but notwithstanding the most careful and extensive search, which I have myself conducted, not a vestige of the Puerco forms has ever been found in these underlying beds. The same is true of the vast stretch of territory along the skirts of the Rocky Mountain divide in which the Laramie is so extensively exposed. Not a tooth, not a scrap nor a fragment of a bone referable to these Puerco Eutherians has ever been found, in spite of the fact that many of the most expert collectors have searched for them assiduously in these localities for many years. It is hardly possible to explain their absence from the Cretaceous on the basis of unfavorable conditions of preservation, for the reason that we have not only the remains of many species of land Vertebrates well and abundantly preserved, but we have Mesozoic mammalian remains as well. The only explanation which seems to me possible is that they were migrants coming into these latitudes for the first time at the beginning of the Puerco.

The same argument applies with equal force to the new elements which were introduced in the Torrejon, Wasatch, Wind River, Bridger, and other epochs. They were simply new-comers representing so many waves or impulses of migration, and the fact that practically the same species were introduced at practically the same time, in the same order, and in the same abrupt manner, in two such widely separated localities as the Eastern and Western Hemispheres, adds overwhelming proof that the two faunæ had a common center of dispersion.

Now, where is it possible to locate such a land area common to the two Hemispheres? Geologists have made out a sufficiently complete history of the continental land masses, as well as of the great ocean basins, to give a tolerably exact idea of the main facts. All are agreed that at no time, at least from the beginning of the Mesozoic to the present, were the broad relations between the oceans and continents, either to the east, west, or south of the localities under consideration, materially different from what they are to-day. There is no evidence whatever of any land connection between the two Hemispheres at or near the Equator at any time in the past, so that the only possible place where this common land area could have existed was in the North.

Let us examine the question again, from the standpoint of the faunal changes which are indicated in the successive deposits of our American Tertiaries. In the Wind River and

Bridger epochs, the general facies of the fauna and flora is that of a tropical or semitropical region, as everyone who has given any attention to the subject is bound to admit. Palms, tropical Invertebrates, Alligators, tropical Turtles, and numerous species of monkeys, give ample proof of the nature of a climate which is to-day so necessary to their existence. In the succeeding Uinta, deposits were made within less than fifty miles to the southward of the typical Bridger localities, and in them we find but few fragments of these tropical or semitropical conditions remaining. The general aspect of the fauna gives evidence of a warm temperate climate, with the appearance of extensive savannas and much open country. In the Oligocene deposits of nearly the same latitude, with the exception of a single specimen of Alligator, no traces of any remains referable to, or characteristic of, even a semitropical climate have been found, notwithstanding the fact that these deposits are among the richest in mammalian remains of any in the world, and have been most thoroughly explored. The monkeys, Palms, tropical Turtles, and Invertebrates had completely disappeared, and there is overwhelming proof to the effect that they migrated to the southward. The European Tertiaries contain the same record, and the evidence that a general southward retreat of both the higher plants and mammals was in progress throughout much of the Cretaceous and the whole of the Tertiary is so complete, conclusive, and incontestable, that it may, in my judgment, be accepted as a fully demonstrated fact.

If, therefore, the sudden introduction of these new elements into the succeeding faunæ and floræ is to be explained upon the basis of migration, we are then in a position to assign a rational cause for its occurrence. We have already seen that the temperature in the region of the Pole was tropical in the early Cretaceous, and that it slowly declined throughout the succeeding epochs until finally a frigid condition was established. The migration of the Puerco fauna we may readily believe to have been caused by the appearance of unfavorable conditions, both climatic and vegetal, which gradually supervened within the limits of its ancient boreal home. In like manner, further modifications of the climate and vegetation in the same region caused the Wasatch exodus, and, like their Puerco predecessors, they had no alternative other than to follow the receding tropical forests to the southward. That some of them remained behind and were gradually modified to fit the new and changing environment, however, there can be also very little question. In this manner and from this cause doubtless arose those types which came finally to dwell in temperate and arctic climates. When changed conditions in the environment

arose, the inhabitants of the region had choice of one of three alternatives; migration, modification to fit the new conditions, or extinction. And I may add that paleontological study concerns itself almost exclusively with deciphering the record of the successes and failures of animal species along these lines.

In the case of the Primates, their structure at the beginning of the Wasatch had been fully adapted to an arboreal existence, as their prehensile extremities so fully demonstrate. Being exclusively dependent on these conditions, they were bound to follow in whatever direction the limits of the tropical forests shifted; and that the recession and gradual retreat of these forests was from within the Arctic Circle in the North to the vicinity of its present confines in the South, during the interval between the Middle Cretaceous and the Glacial epoch, is supported by an overwhelming abundance of evidence. It thus becomes possible, it seems to me, to locate the place of the origin of a large number of the higher Mammalia. Whether a similar series of changes took place at the South Pole, giving rise to its own peculiar types of mammals, is as yet uncertain, but at the same time not improbable. At all events, the Primates belong to the North, and it is within the Arctic Circle that they had their beginning. *

The second inquiry with which we are at present concerned is the kind and character of mammal from which the Primates were probably derived. Unfortunately the evidence necessary for a final solution of this problem is very far from complete. The question of the oldest known Primates has not yet been settled with satisfaction. Certain species from the lower Tertiaries of this country, notably the Puerco and Torrejon beds, have been variously referred to the Primates and Creodonta by investigators who have studied them—Cope, Scott, Osborn, Earle, and Matthew. These animals are known almost exclusively from the teeth, and in a few instances only are any of the

* In this connection I wish to call attention to the admirable work of Hon. G. Hilton Scribner, New York, 1883, "Where did Life Begin?" It is now quite impossible to state by whom this view was first entertained, but the late Professor Asa Gray, in a private letter to Mr. Scribner in 1884, distinctly claimed the credit of having been the author of the migration on north and south lines. Reference to Gray's published writings, however, as well as to those of Saporta, Nathorst, and Dawson, fails to show that any attempt was ever made by any of these eminent investigators to put forward the proofs and formulate the view into a distinct hypothesis of the Polar origin of life. In this Mr. Scribner was clearly the first, and so ably and logically was the Polar Origin theory presented, that Dr. Gray in commenting upon it was led to remark that Mr. Scribner's position was "simply incontestable." In the light of the great mass of evidence now at hand, it is easy to recognize and to appreciate fully the extreme probability of such a view; but when we recall the almost total lack of evidence in its favor at the time Mr. Scribner's book appeared, his work becomes all the more noteworthy, and in my judgment is entitled to a high rank among intellectual performances of a similar kind. This should thus be known as the Scribnerian Theory of the place of the origin of life.

other skeletal parts represented. In comparison with the oldest and most primitive undisputed monkeys, a careful examination of these Puerco and Torrejon forms reveals a wide difference in the organization of the two, and it is much to be doubted whether they really have anything in common with the true Primates. It appears necessary, however, to make an exception to this statement in the case of the genus *Mixodectes* and possibly also of *Olbodotes*. Matthew believes,* from an associated astragalus, that the former of these genera belongs to the Rodentia rather than to the Primates, but its successors in both the Wasatch and the Bridger show very strong Primate affinities; hence, there can be little doubt that they constitute an aberrant side branch of the main Primate axis.

If we thus exclude the *Mixodectes-Microsyops* series, the oldest remains of true and undisputed Primates occur in the Wasatch, and there are excellent reasons for the belief that they were new types of sudden introduction in the region, at the beginning of this epoch. Unfortunately, these Wasatch forms are very imperfectly known, so that it is not until we reach the Bridger that anything like complete skeletons have been obtained. Some, at least, of these Bridger types, while true monkeys, were in practically the same state of evolution as many of the existing lemurs, so that by careful comparison of the two groups we are enabled to eliminate the specialized and advanced features and reach the more fundamental primitive characters which must have belonged to the ancestors of all Primates.

There has been relatively little speculation on the definite and more exact origin of any of the great groups of Eutherian mammals, and apparently less on the Primates than any other. Huxley, in his famous paper "On the Application of the Laws of Evolution to the Arrangement of the Mammalia," expressed the opinion that they arose from a central type of the Insectivora, such as the Erinaceidæ. Nearly twenty years later we find no less an authority than Hubrecht† warmly supporting the same view. In speaking of *Erinaceus* and *Gymnura*, Huxley's exact words are: "in them, even more than other Insectivora, we possess the key to every peculiarity which is met with in the Primates, Carnivora, and the Ungulata." However much I may be disinclined to dissent from the views of so great a master in morphology, we can not disregard the fact that the very large increase in our knowledge of the extinct forms during the past twenty years has materially altered our conceptions of the relations of these groups. It is true that the Insectivora furnish

* Bull. Amer. Mus. Nat. Hist., 1897, p. 265.
† Descent of the Primates, p. 5, 1897.

a type of cerebral circulation which might easily have passed into that of the Anthropoidea, through the suppression and disappearance of the stapedial branch of the entocarotid; but, as we have already seen, this character is shared by the Rodentia and probably by other groups as well. At the same time it does not furnish a type of cerebral circulation from which that of the lemurs could have been evolved.

The greatest difficulty in the way of deriving the Primates from any form or forms of the Insectivora at present known consists in the total lack of prehensile powers of the manus or pes. Any group which is placed ancestral to the Primates must of necessity be one in which some distinct approach to this condition is made, since its possession is one of the chief requisites of fundamental importance. For this reason, if for no other, the modern types, at least, of the Insectivora, can not be looked upon as the forerunners of, or as more than very distantly related to, the ancestors of the Primates.

With the single exception of *Lophiomys* among the Rodentia, the only other living mammals which exhibit prehensile extremities are found among the Marsupials, and the evidence points very conclusively to the fact that all of them, even those with highly modified limbs for terrestrial progression (as the kangaroos), are descended from ancestors with grasping hands and feet. It is therefore not beyond legitimate supposition to assume the existence of a very considerable group of ancient Metatherians living within the Arctic Circle during Cretaceous time, whose manner of life had already become arboreal. If such a group did exist, it is far more likely that the Primates were derived from it rather than from the Insectivora or any other group now living. Future research and discovery in these regions will alone settle the correctness or incorrectness of this hypothesis, and until such discoveries are made we must be content with the results of speculation.

[To be continued.]

ART. XLIII.—*The Geological Structure of the Southwestern New England Region;* by WILLIAM HERBERT HOBBS.

Former Assumption of Deformation largely by Folding.

THE southwestern New England region, by reason of its prominence in the early heated discussions upon the stratigraphic and dynamic problems of geology, has sometimes been designated "The Battlefield of American Geology." Its prominence in this controversy is to be explained chiefly on two grounds—its proximity to the early American colleges and the exceptional intricacy of the problems involved in its study. The first condition is responsible for the large number of geologists who figured in the Taconic Controversy; the second for the wide range of opinions expressed, for where little can be learned by observation much may be assumed, though not without challenge. It is certainly most unfortunate that the solution of the most fundamental problems of the science, in their application to America, should have been sought in a region which probably ranks with any yet known both in the intricacy and in the obscurity of its geological structure.

All work earlier than that of Dana may be to-day passed over as offering nothing of value upon the geological structure of the region. Charles Hitchcock and Ebenezer Emmons in Massachusetts did, indeed, prepare some careful sections across the western mountain ranges, and these sections fail to be of service to-day not from any lack of ability or of thoroughness on the part of the excellent observers, but because any work so limited and carried out upon ideas of rock structure which were current at the time is wholly inadequate. Percival's work in Connecticut was essentially areal mapping, and while his observations of petrographic characters and his correlation of exposures to form rock belts must challenge our admiration, he was yet too wise and too cautious to risk any dogmatic statements regarding the tectonic structure of the region. It is clear, however, that he believed the crystalline rocks to be given their present attitudes as the result of system of flexures.*

Dana by most careful work, chiefly areal, but in part structural also, extending throughout the area from Vermont to Long Island Sound in New England and in much of eastern New York, was able to solve many of the problems which had vexed the early school of geologists, even though he was too

* Percival, Geol. of Connecticut, New Haven, 1842, pp. 289–298.

deeply involved in the controversy to settle them. Walcott, by his discovery of fossils in beds hitherto supposed to be barren of them, was able to review the early questions of stratigraphy and to end forever the Taconic Controversy.

The problem of the areal delimitation of horizons within the New England area and the determination of the geological structure of the region remained, and the early dissensions had at least shown the necessity for a much more thorough survey of the region than any which had been attempted. The work of the U. S. Geological Survey within the region, begun before that of Dana had been completed, was therefore taken up with a plan more comprehensive than any other and on a scale more nearly what was demanded by the conditions of the problem.

In view of the difficulties which have been encountered in the progress of this work and the methods of investigation which experience has shown to be of value, it is well to consider for a moment the fundamental assumption of Dana in his study, and also the dominant idea maintained throughout all the earlier work of the U. S. Geological Survey. Dana in his later papers summarizes the general geological characteristics of the Taconic region and says of the structure:

"The rocks owe their positions to a system of flexures, and the folds are to a large extent overthrust folds."*

It is evident from his sections that it was not Dana's intention to express by the term "overthrust folds" ruptured folds, but folds with inclined axial planes. In his summary he makes no mention of faults, and though a fault is indicated in one or two of the sections, it is evident that faulting hardly figured in his conception of the structure of the region.

Essentially the same opinion was held by Pumpelly, who was in charge of the work of the U. S. Geological Survey during the earlier years, when the Hoosac-Greylock province in northwestern Massachusetts and southern Vermont was surveyed. With him, however, the supposed absence of faulting within the area was clearly expressed as the reason for assuming a deformation wholly by folding. In his report he says:

"It was evident that our first and hardest work would be to find the key to the structure of the region. For this purpose I sought a region where the western edge should present, instead of a straight line, as many bay-like curves as possible, and where the structure of the ridge itself should show folds with pitching axes. I hoped in such a region to eliminate the difficulties introduced by possible faults, as well as the temptation to infer their existence; and also, in the case of pitching folds, to get through

*James D. Dana; On Taconic rocks and stratigraphy, with a geological map of the Taconic region. This Journal (3), xxix, 209, 1885.

radiating cross sections a knowledge of the true order of bedding."*

There have been few geologists more thorough and conscientious than Raphael Pumpelly, and the realization of the danger of inferring faults is perhaps a characteristic of all the best American geologists of the recent past and of the present alike. It ought, however, to be pointed out that the danger of inferring faults where they are absent, is hardly more serious than that of assuming their absence where they are present. The present attitudes of the rock beds must be accounted for either by the one or the other kind of deformation or by the two in conjunction, and these attitudes can in most cases of difficult areal geology be accounted for upon either theory. It is also worth noting that the edge of a ridge which shows many "bay-like curves" is just the kind most likely to be caused by a series of parallel and intersecting faults; and the presence of folds with pitching axes, while offering a possible and perhaps adequate explanation for the alternation of formations in the direction of their strike, does not necessarily reveal the real or the only actual cause.

This assumption of the absence of faulting throughout the New England area has been fundamental and far-reaching in the work of the Survey, as is shown by the fact that in the area of 200 square miles mapped with great detail and described in the report cited, but a single fault is indicated, and this a strike fault, presumably an overthrust. As the work has been extended to the south the same tendency to resist the temptation to locate faults has been manifest, and from his own experience the writer can testify that in twelve seasons of independent field work, or until the Pomperaug Valley area of Newark rocks was examined, not a single normal fault was entered upon his maps. It is believed that the tendency has been hardly less marked in the work of other members of the division, and it might almost be added, for the most careful work upon the crystalline schists throughout the country. Indeed it would hardly be too much to say that structural studies of the crystalline areas of the United States, with the single exception of the Great Basin region, have been carried out upon the assumption that rock deformation takes place by one process only, namely, by crustal folding and the thrusts incident to folding. Even in the classical area of the Great Basin the attempt has recently been made, though unsuccessfully, to explain the structure by a system of folds alone. This marked trend in American geological work may be in no small measure due to the classical geological studies of the

*Pumpelly, Wolff, and Dale; Geology of the Green Mountains in Massachusetts. U. S. Geol. Survey, Mon. xxiii, 7, 1894.

Rogers brothers, who by their explanation of Appalachian structure on the basis of folding alone, not only established a new type which bears that name, but developed methods which have been left as a legacy to their successors.

In Europe, on the other hand, the principles so long ago worked out by Kjerulf, Sedgwick, De La Béche, John Phillips, and especially by Daubrée* have been utilized to such an extent that the recognition of systems of faults as important elements of crustal deformation may be truly said to be the most marked line of cleavage which separates European structural studies from American. It has been especially the work of Suess to correlate these scattered studies and to show that the lineaments of the continents are lines of normal faulting, between which great orographic blocks have been depressed by different amounts. His monumental work upon The Face of the Earth† has been the greatest interpretative work upon structural geology of the past quarter century, and marks an epoch in the history of the science. In the Principles of North American pre-Cambrian Geology, the most comprehensive American treatment of rock deformation, Van Hise devotes 75 pages to a consideration of deformation by folding, whereas 10 pages suffice for a treatment of jointing and faulting inclusive of thrust faults which are connected with folds. There is no doubt that the proportion of space thus devoted to joint and fault structure is far beyond what its consideration by American geologists would warrant.

Inadequacy of Hypothesis of Folding.

As respects the study of the southwestern New England area, it is, I believe, time to admit that the basal assumption of a deformation by folding alone, which has now been given a trial by the Survey during a period of nearly twenty years, has proven entirely inadequate. Where belts of limestone are present to serve as guides in the areal and structural work, the order of superposition of formations, and in a very general way the geological structure, have been made out with a reasonable degree of certainty. In the broad belt of schists and gneisses which lie to the eastward of the limestone, on the other hand, it has thus far been found impracticable to discover any structure regarding the presence or absence of which any two of the workers can agree. This is true in spite of the fact that the geologists in the field have acquired by long experience a familiarity with the varied petrographical types of the province and with the apparent structures developed in

* Daubrée, Géologie experimentale, pp. 304–306, Paris, 1879.
† Antlitz der Erde, two vols., Prag and Leipsic, 1885.

them, and are, moreover, by reason of the most cordial personal relations desirous of securing perfect harmony of view. From examination of himself, no less than of others, the writer is inclined to believe that confidence in any structure worked out in the region upon a doctrine of deformation by folding alone is more largely based upon psychological than upon geological conditions; and this for the reason that a feeling of confidence inspired by observation at one locality has too often been quickly and most effectually dispelled by the next succeeding observation at an adjacent exposure. In the study of the structure within the gneiss belts there are two main tendencies which in varying proportions will be found to characterize the work of different geologists. One tendency is to group together within a single formation a considerable number of variant types of gneiss because no apparently satisfactory basis for their separation is at hand; and as the work progresses it is found that all the types of one formation can be duplicated in the others. The other tendency is to make an elaborate differentiation of units on a petrographical basis, with the result that extreme difficulty is met with in accounting for the present attitudes of the rocks, and large drafts must be made upon the imagination. At different points between the two horns of the dilemma different workers will very naturally be ranged.

The Newark Areas in their Relation to the Crystalline Belts.

In the summer of 1899 the writer, in surveying for folio publication the areas for 30′ quadrangles, mapped with much detail the circumscribed area of Newark rocks lying within the basin of the Pomperaug River in Connecticut. These formations of the Newark system present markedly contrasted petrographical types, are practically unaffected by flexuring or metamorphism, but exhibit in the most beautiful manner the results of a deformation by jointing and normal faulting. After long attempts to decipher the obscure and illegible characters in the structure of the surrounding gneiss belt, the transition to the clearness and simplicity of the record preserved within the Newark basin was like the passage from darkness to light. The study of the Newark area showed that it was deformed by a complex system of parallel and intersecting faults all near the vertical, and that the system of joints which preceded and conditioned the system of faults was with little doubt the result of compressive stresses which affected the southwestern New England province as a whole. The principles underlying such a deformation by fracture as a result of compressive stress have been given a somewhat full exposi-

tion by Becker* and were extended and applied to the area in the writer's report upon it.† The greatest value of the investigation of this Newark area, as it relates to the structural problems involved in the study of the crystalline belt, has been to show that no adequate explanation can be offered for the present attitudes of the rocks within that belt which fails to take account of a deformation by normal faulting as well as by folding, for it would be absurd to suppose that the faults characteristic of the Newark areas are not extended beyond their margin. Indeed, the evidence that they pass far beyond these margins into the crystallines has been shown by their direction of the courses of streams.‡ A somewhat careful review of the studies made of the other Newark areas§ of the Piedmont plateau has shown that while systems of parallel joints and faults had not been generally recognized as such, with not an exception numerous intersecting faults of the normal type had in each area been observed, and all recent observers have further expressed the view that many other faults remained concealed. The obvious lesson from these considerations is that the deformation of the intervening areas of crystalline schists—and these areas are not large in proportion to the Newark basins—must be not only by folding and probably also by faulting in pre-Newark time, but there must have been superimposed upon the earlier structures the system of jointing and faulting of post-Newark time. It is probable that the throws along the faults of this system outside the Newark basins will in general not be as great as those within the basins, for the reason that the latter are depressed areas within the crystalline terrane, but it will hardly admit of doubt that they have been important enough to produce a mosaic of orographic blocks in which different beds will be placed in juxtaposition along essentially vertical walls at the present surface. Another result of the study of the Newark areas has been to confirm the views of some writers that displacements are seldom concentrated upon a single plane of faulting but are usually distributed over a number of parallel planes lying in a zone more or less extended. Such distributive faulting renders more difficult the observation of individual fault planes, and even when these are discovered it is apt to leave an erroneous impression of their relative importance.

*George F. Becker, Finite homogeneous strain, flow and rupture of rocks; Bull. Geol. Soc. Am., iv, 50, 1893.

†The Newark system of the Pomperaug valley, Connecticut, Twenty-first Annual Report of the Director of the U. S. Geological Survey, Part III, pp. 1–160.

‡The river system of Connecticut; Jour. Geol., ix, pp. 469–484 (1901).

§On the former extent of the Newark system; Bull. Geol. Soc. Am., xiii, pp. 139–148, 1902.

Methods of Investigation.

Convinced, then, of the presence of a system of faults within the crystalline belt, the writer's next problem was to determine, if possible, its relation to the system of joints, the direction of its component series of parallel faults, and the part which it has played in the deformation of the area. The difficulties in the way of reading at once the composite structure of folding and faulting in rocks so nearly of one petrographical type as are the gneisses of the region, seemed, in the absence of established methods, to be insuperable, and attention was therefore first directed to those parts of the area in which limestone occurred. In the search for evidence of the system of faults within these areas the most hopeful factor was that rectilinear surface elements are characteristic of joint and fault structures, which are in contrast with the curves characteristic of folding. To this was added the knowledge derived from study of the Newark areas that the post-Newark system of faults, at least, had not been affected by subsequent folding.

It may be stated that based upon this and other known peculiarities of faults, methods more or less decisive have been developed which have led to the determination of elements in the fault system of the region. These methods have been elsewhere described* and can be but briefly referred to here.

Some of the indications of faults are:

1. Beds formed at different times in juxtaposition along a plane transverse to their bedding.
2. Offsetting of formations in outcrop.
3. Offsetting of outcrops as definite topographic features.
4. Dikes.
5. Abrupt changes of strike and dip not indicated in the folds.
6. Fault breccias.
7. Slickensides.
8. Abrupt disappearance of outcrops along a markedly rectilinear boundary.
9. Occurrence of scarps in the more resistant rocks.
10. Fault gorges.
11. Arrangement of surface springs in rectilinear directions.

It was further realized that it would probably be difficult to locate more than a small proportion of the individual faults occurring within the district, and effort was therefore made as early as possible to learn the distinguishing characteristics—especially the bearings of the individual fault series—of the

*The Mapping of the Crystalline Schists, I, Methods; II, Basal Assumptions. Jour. Geol., x, pp. 780–792, 858–890, 1902.

fault system. Additional observations of value for determining the system of faults as a whole are:

1. The arrangement of rectilinear formation boundaries in a network of parallel series.
2. The arrangement of individual faults in intersecting parallel series.
3. Determination of the joint system and its relation to the fault system.
4. Zigzag topographic relief composed of straight elements in parallel series.
5. The drainage system a network of intersecting parallel series.

With abundant outcrops a fault system may be disclosed upon the areal map by the generally rectilinear or zigzag boundaries of formations. If not, some indication may be afforded by the prevalence of straight lines and sharp bends in the topography, particularly if the lines fall into intersecting parallel series. Such a relief may not be apparent without careful examination of the map, for the reason that the particular combination of directions is not known, and it may yet be quite striking when once discovered. Again, the drainage lines may compose a network which in direction conforms both to that of the topographic lines and that of the formation boundaries. The prominent joints observed at the individual exposures, as was long since shown by Daubrée, will, if of the same origin as the fault planes, conform in direction to the directions of these planes.

Application of Methods.

In applying the above principles to the structural study of the crystalline belt in southwestern New England, advantage has been taken of two important considerations.

1. *The areas of most intricate areal development of the formations are key areas for determination of the manner of deformation.*—In geological mapping it is almost an unwritten law that geological sequences must be established in areas where formations appear in their larger masses at the surface, it being generally assumed that structural relationships are in such cases the simpler. Those areas, on the other hand, which exhibit a considerable number of formations brought closely together in small masses at the surface are apt to be looked upon with suspicion as areas of local and so-called "minor" faulting, or as containing intercalated beds of unusual types due to purely local conditions of sedimentation. Whichever of these views be assumed, the areas are likely to receive but

small attention: first, because the problem of their structure is difficult to solve and perhaps is not regarded as affecting the larger questions of the region; and, second, because if once solved the scale of the map would not allow of its representation. Such areas are therefore more frequently represented upon the map in the color of the formation which is believed to compose their greater part.

However unsuited these intricate areas may be supposed to be for establishing the order of succession of geological formations, they are, nevertheless, it is believed, in many cases the keys, and in some cases the only ones, to unlock the secret of the manner of deformation which has affected the region as a whole. Complicated though these problems may be, they often require only patience and industry for their solution, whereas the larger masses by their very simplicity of areal distribution allow of several hypotheses, any one of which would explain them.

Four areas of exceptionally intricate areal relations and widely separated in the region were selected for study. They are the area of sharp ridges near Lee, Massachusetts; Evergreen Hill in Stockbridge, Massachusetts; the Sheffield-Salisbury area; and the Greater New York area. Between the two last mentioned is located the Pomperaug valley area, which, from its detailed mapping, may be counted as a fifth. The Greater New York area has been discussed in papers before the New York Academy of Sciences* and the Geological Society of America.†

After working out so far as possible the local conditions of deformation within the circumscribed areas above mentioned, the larger surrounding areas have been studied not only with respect to the kind of rock and the inclination of its plane of lamination at each locality (as usually done), but the broad distribution of outcroppings, the direction of outcrop margins, the lineaments of the landscape as revealed in topographic relief and drainage lines, and the directions of joints and faults, have all been considered in their relation to the structure disclosed within the smaller areas.

2. *Fault structures are best preserved by the more resistant rock masses.*—Revelations of fault structure through the topographic relief are most likely to be made in areas where the more resistant rock masses are at the surface. The perfection of fault structures preserved in the Newark areas is in no small measure accounted for by the presence there of the dense resistant basalt masses. So resistant a type is not found within

* See review of this paper in Science, xvi, pp. 905–906, 1902.

† Read at Washington December, 1902. See review in Science, xvii, 298 (1903).

the crystalline belt, but some approach to it is afforded by a zone of silicified limestone or dolomite lying between Sheffield, Massachusetts, and Falls Village, Connecticut. This zone of silicification has been supposed to be accounted for by fracture and infiltration along a fault and has already been designated the Housatonic fault.*

Conclusions.

As a result of the studies of these areas it may be stated that, based upon the peculiarities of faults and of fault systems as outlined above, an abundance of evidence has been discovered to show that the area of southwestern New England has been deformed by a system of joints and faults of post-Newark age superimposed upon older structures which appear to be largely due to folding. The system of faults conforms in direction with the system of joints and is in fact occasioned by a displacement which has occurred along the joint planes. The evidence of this can only be presented with the aid of detailed maps of the several areas accompanied by full descriptions and illustrations. The present paper can therefore be considered only as a preliminary notice of a larger work to appear in a professional paper of the United States Geological Survey. The relative importance of the fold structure and the superimposed fault structure it is difficult to estimate quantitatively. It appears, as expected, that both have played their part, but the result of the investigation has been to show that, so far as the areal distribution of formations and the directions and positions of their mutual boundaries are concerned, the rôle of the system of folds has been altogether subordinate to that of the system of Post-Newark faults.

Madison, Wisconsin.

* Jour. Geol., i, pp. 793–798 (1893).

Art. XLIV.—*A New Machine for Tracing Speech Curves;** by E. W. Scripture.

A grant from the Elizabeth Thompson Science Fund rendered it possible to construct a machine to trace off the speech curves on the celluloid cylinders of the Lioret phonograph, and to thus extend to the study of French the methods of work already applied to English and German.†

The celluloid cylinders are the invention of Dr. H. Lioret of Paris; they are practically indestructible. Those of Model A are 55mm in diameter and 43mm in length. The speech groove consists of depressions just as in the ordinary wax phonograph. They reproduce the voice with great truthfulness. For tracing the speech groove we remove the cylinder from its brass frame and place it on the rotator (fig. 1). This is a steel barrel having a tapered end for the cylinder. It is rotated by a motor. The speed of the motor is twice reduced to $\frac{1}{180}$ by combinations of a worm and a 160-tooth spur gear, and finally to $\frac{1}{3}$ by the pulleys, giving a total reduction to $\frac{1}{28800}$. The speed of the motor is so adjusted that the cylinder turns once in $4\frac{1}{2}$ hours.

As the barrel turns, it is made to move axially by a thread turning in a brass nut. An additional bearing takes the strain of the pulleys and relieves the nut. The celluloid cylinder thus turns and moves axially in such a way

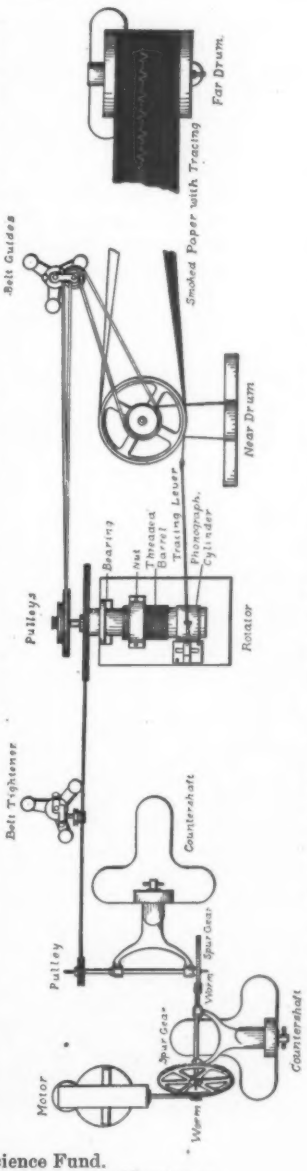


Fig. 1. Tracing Machine for the Lioret Phonograph Records. (Top View; scale, 1 : 12.)

* Elizabeth Thompson Science Fund.

† Scripture: Experimental Phonetics, Chapter iv.

that the speech groove passes under a sapphire point which follows the rise and fall in the bottom of the groove and moves a light tracing lever. The point of this lever records the movement on a long band of smoked paper passing over two drums. The near drum is run by a belt directly from the rotator. The speech curve appears in great magnification on the band of smoked paper. When the band has gone once around, the record is fixed by brushing the *back* of it with an alcoholic solution of shellac; this fixes the smoke from the back and produces a mat surface.

The tracing lever is the critical part of the apparatus. Fig. 2 shows the end view of the rotator with the phonograph cylinder. The sapphire at the end of a steel point follows the rise and fall of the speech groove. The fulcrum of the lever

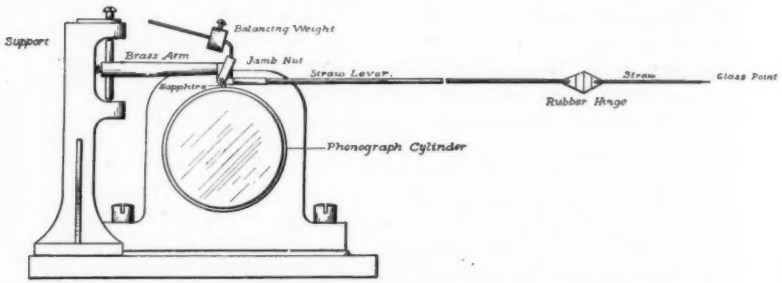


FIG. 2. Tracing Mechanism. (Side view; scale, 1 : 3.)

to which it is attached is just to the left of it. The long arm is made of two selected German straws of unusual lightness and rigidity. This is balanced by a weight. The end of the straw arm carries a somewhat modified Baylis point. This consists of two small bits of cardboard united by a hinge of the finest rubber membrane. One piece is attached to the tracing lever, while the other carries a piece of light French straw with a minute glass ball at the end. This form of recording point gives a minimum of friction.

Since there is no springiness in the rubber hinge the glass point will not remain in contact with the paper. Springiness was attempted by putting a fine hair across the hinge; the result was not satisfactory, as too light pressure would allow the point to leave the drum when the speech curve deviated from the vertical plane, and too heavy pressure diminished the amplitude. By slightly tipping the hinge gravity could be made to keep the point on the paper. Since it is impossible to get the cylinder on the rotator so that the speech grooves lie in absolutely vertical planes, some arrangement must be made

for side motion. This is done by supporting the tracing lever at the end of a brass arm which is pivoted in vertical bearings.

The delicacy of adjustment of the bearings of both the tracing lever and the brass arm must be very great. The methods of testing and adjusting have to be acquired as an art, and a description would be both tedious and useless. The delicacy of the recording point is so great that if one finger is placed on the metal base of the rotator and another on the very stout upright support bolted rigidly to the base, the recording point will indicate the yield of the two pieces of metal.

For tracing paper it has been found best to use unglazed box paper. A careful test showed that with the Baylis point there was no diminution in the amplitude of the waves due to friction on the paper.

To avoid jarring, the whole apparatus is placed on a board which is suspended from the ceiling by cords attached to springs.

A specimen of the tracing of a French vowel is shown in natural size in fig. 3.

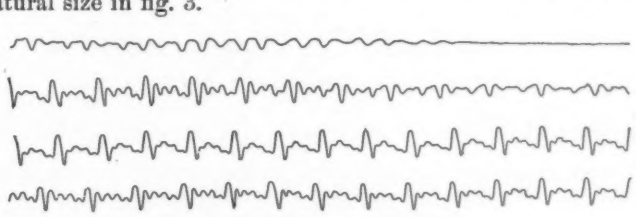


FIG. 3. Vowel Curve. (Scale, 1 : 1.)

By applying to M. Henri Lioret, 12 rue Thibaud, Paris, a record of any person will be made to order at the regular price of 2 frs. 50. It would be quite feasible for any one in Paris who is interested in French dialects to bring the proper persons to the above address and have the cylinders made. They could then be traced off by the machine and the results delivered to the person ready to study them. I have had a number of records made by one person, including *Le Roi d' Yvetot*, *Le Corbeau et le Renard*, *Anecdotes linguistiques*, *Lafon et l'Amateur*, *Amusettes phonétiques*, *Sur un Mort*, and portions of *Le Bourgeois Gentilhomme*.

This machine has now traced off a series of words containing typical French sounds. As these results fully occupy the one person for whom they were made, I am ready to trace off any of the cylinders already on hand or any other cylinders for any one who wishes to prepare the results for publication. As no accurate curves of French speech have ever been made, these results would have immediate value.

Yale University, New Haven, Conn.

ART. XLV.—*New Forms of Sperrylite;* by VICTOR GOLDSCHMIDT and WILLIAM NICOL.

THE sperrylite examined by us during the summer of 1902 was obtained at the Vermillion Mine, Algoma District, Province of Ontario. This is the original locality from which was obtained the material examined by Wells and Penfield* and that described by Walker.† By "panning" the loose material at the mouth of the old shaft a heavy residue was obtained. When this residue was examined by a needle and pocket lens the brilliant silver-white crystals of the mineral were readily separated from the magnetite, etc., occurring with them. The crystals are exceedingly small, but on account of their brilliant metallic lustre the measurement of the minute faces was easily possible. The crystals were measured on the two-circle reflecting goniometer, making use of the reduced signal.

Nine crystals were measured, which showed the following forms:

	Crystal System Regular.						**Pentagonal Hemihedrism.**								
Letter:	c	*a	*g	e	*h	*b	*d	*k	*m	*q	*B	p	*u	*ψ	*x
Symb. Gdt.:	0	$+\frac{1}{0}$	$-\frac{3}{0}$	$+\frac{1}{0}$	$+\frac{3}{0}$	$-\frac{3}{0}$	-10	$\frac{1}{4}$	$\frac{1}{3}$	$\frac{1}{2}$	$\frac{3}{4}$	1	$1\frac{1}{2}$	$\frac{11}{11}$	$-\frac{2}{3}$
Symb. Miller:	001	103	$\bar{2}05$	102	305	$\bar{2}03$	$\bar{1}01$	114	113	112	335	111	212	214	$\bar{2}13$

Of these forms, those designated with an asterisk are new for sperrylite. In addition to these, two other forms,

$$?\,z = +\tfrac{3}{5}\tfrac{1}{5}\,(315); \quad ?\,D = +\tfrac{1}{2}\tfrac{1}{3}\,(326),$$

were observed, but not with absolute certainty.

Charles W. Dickson,‡ doing post-graduate work at the School of Mines, Columbia University, New York, has succeeded in separating sperrylite crystals from the unaltered chalcopyrite from the Victoria Mine, in the Sudbury District. He reports by letter, "I found, on examination of two of the best crystals, the trapezohedrons 211 and 411, besides the originally described forms."

The form e occurs on all the crystals measured, having only half the number of faces developed, in accordance with pentagonal hemihedrism. This form has been regarded as *positive*, and the *positive* or *negative* character of the other forms has been determined in accordance with this. This criterion may be accepted as decisive. On account of the minute size of the crystals it was not possible to attempt to decide this point by etch-figures.

* This Journal (3), xxxvii, 67 et seq., 1889. Zeitschr. f. Kryst., xv, 285 u. 290, 1889.

† This Journal (4), i, 110 et seq., 1896. Zeitschr. f. Kryst., xxv, 561, 1895.

‡ This Journal (4), xv, 137, 1903.

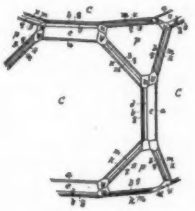
The nine crystals measured showed the following combinations:

Combinations.

Crystal No. **1**:	c	.	g'	e	.	b'	.	.	m	q	.	p	u	.	.	.	.	Fig. 2.
Crystal No. **2**:	c	.	.	e	.	.	.	.	.	.	.	p	.	.	.	.	.	
Crystal No. **3**:	c	.	.	e	.	.	.	.	.	q	B	p	.	.	.	.	.	
Crystal No. **4**:	c	.	.	e	.	b'	ð	.	m	q	B	p	u	ψ	x	.	.	Fig. 3.
Crystal No. **5**:	c	.	.	e	.	.	ð	k	.	q	.	p	u	ψ	.	(z)	.	Fig. 4.
Crystal No. **6**.	c	.	.	e	h	.	.	.	.	.	.	.	.	.	.	.	.	Fig. 5.
Crystal No. **7**:	c	.	.	e	.	.	ð	k	m	q	.	p	.	.	.	.	(D)	
Crystal No. **8**:	c	a	.	e	.	.	.	.	.	.	.	p	.	.	.	.	.	
Crystal No. **9**:	c	.	.	e	.	.	.	.	.	q	.	p	.	.	.	.	.	

From this table of combinations the relative frequency and importance of the individual forms can be observed.

1



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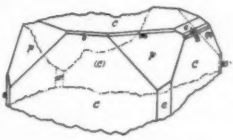

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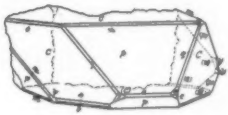


The following arrangement shows the relative importance:

c always present, usually predominating.
p almost always present, usually smaller than **c**, sometimes of equal importance with **c**, now and then predominating.
e always present, but less important by far than **p**.
q ð u m very small, but occurring not seldom.
a g' h b' k B x occurring seldom and unimportant.
z D scarce, faint and uncertain.

Up to the present time, from the measurements of Penfield and Walker, the following forms were known:

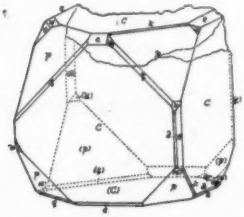
$c = 0\ (001)$; $\delta = 10\ (101)$; $e = +\frac{1}{2}0\ (102)$; $p = 1\ (111)$.

These are, in fact, the most important forms. In addition Walker gives the form $\frac{1}{2}\frac{1}{5}$ (5.2.10), determined from measurements of plane angles. This form was not found on any of the nine crystals measured. It lies in the zone $+\frac{1}{2}q$, between $e = \frac{1}{2}0$ and $q = \frac{1}{2}$,—an important zone for this mineral. It may perhaps be identified with the form $\psi = \frac{1}{2}\frac{1}{4}$. In the same zone lies the uncertain form $D = \frac{1}{2}\frac{1}{3}$ and the well defined form $u = \frac{1}{2}1$. Figures 2, 3, 4 and 5 show crystals Nos. 1, 4, 5 and 6. Fig. 1 gives an ideal picture of all the forms. Owing to the difficulty of drawing so many narrow faces and reproduc-

ing them by lithographic plate, only part of the crystal is shown. The forms x and h are wanting, as their existence was decided with certainty by measurements made after the drawing was prepared. In figs. 2–5 the attempt has been made to preserve the proportions of the faces as they actually occur in nature.

The measurement, or rather *the discussion of the results of the measurement*, offered several difficulties which can be referred to the small size of the crystals and especially of the subordinate faces. These faces are not only absolutely but also relatively small, that is, when compared as to size, with the principal faces, c and p, they stand far in the background. This may be readily seen from an inspection of the figures. In discussing the measurements in the gnomonic projection,

4



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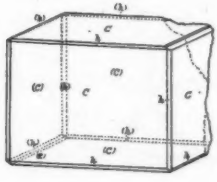


the following unexpected phenomenon was observed. In cases where the principal faces c and p occupied their exact position, showing that the crystal had not been disturbed in its growth, yet the signal reflections from the subordinate faces were displaced in many ways, and this, too, when these faces were well defined as to lustre and perimeter. The minuteness of the subordinate faces is exceptional. It is interesting to note what minute faces can be measured by the excellent instruments of the present day. The following measurements were obtained under the microscope:

Crystal No. 4, Fig. 3.

The large upper c face,	longest diameter	= 0·8	mm
The large front p face,	"	= 0·6	
The upper e face, to the right from p,	"	= 0·06	
The lower ψ face, to the right from p,	"	= 0·03	
The lower x face, to the left from p,	"	= 0·03	
The lower δ face,	length = 0·3 mm ; width = 0·015		

The displacements and consequent variations in the case of such small faces may have two causes:

1. Optical displacement and diffusion of the signal reflection owing to bending of the light. The smaller the face the stronger is this effect and the less the certainty in determining the position of the face.

2. Curvature of faces near the outlines. This is a consideration of importance in connection with the origin of the crystal and demands special study. Sperrylite itself may perhaps furnish suitable material for this on account of its perfect metallic lustre, which makes it possible to secure good signal reflections from even minute faces: on account of the simplicity of the crystal system and the certainty of the elements and calculated angles: on account of the perfect formation of the principal faces, the great variety of kinds of faces and the large number of individual faces, which are incident to the regular system. It may be said: *the degree of perfection of a face is a function of its absolute and relative size.* Very large faces are usually imperfect since their parts and the parts of the layers underneath have a development history exposed to different influences. The faces of smaller crystals are, as a rule, better formed than those of larger crystals. Below a certain size the faces again become more imperfect. This may be assigned to two causes:

1. The relatively small faces are those derived from higher complication. They are weak and more easily displaced. They are mobile, like the leaves and branches in the wind, while the trunk is unmoved.

2. In the case of small faces the marginal parts are relatively large. The marginal parts, however (on edges and corners), are the places at which the particles are bounded only on one side by securely situated particles which are, as well, oriented themselves and have an orienting influence on their neighbors. This is the place of the manifold formation of varying resultants by complication.* The midfield of not too great a face is therefore usually the smoothest. Convexity readily occurs at the perimeter and corners. Small faces lying close together and very little inclined towards one another, frequently shade into each other by rounding. It may happen that *only* the border part is present, no smooth middle part has been formed. If, now, the border part is large relatively, it furnishes much light for the signal reflection. In this way the signal reflection from the inner portion of the face, when this is small and of low brilliancy, may become darkened and eclipsed by that of the border parts. If the faces are too small it is not possible to shut off the border portions by means of the shutter at the eye-piece of the telescope. It is not advisable at this place to go more deeply into these interesting points. They should form the ground of a special investigation.

The discussion of the position of the faces and the symbols of the subordinate ones must be undertaken with caution on the above mentioned grounds. Considerable experience is

* Compare Zeitschr. f. Kryst., xxix, 47, 1897.

necessary in order to be able to separate here the certain from the uncertain. One must also, by measuring numerous crystals, accustom one's self to the peculiarities of the kind of crystal at hand. On the one hand, there is the temptation to symbolize the positions of the diffused signal reflections. This may lead to great confusion. On the other hand, there is the danger of demanding too great precision, and therefore of finding here only the principal forms c, p, e. It was attempted by careful discussion of the measurements and their picture in the gnomonic projection, to obtain as many certain and typical forms as possible. The forms here given as certain may be accepted. Those indicated by (?) viz. z and D, possess a high degree of probability. Under these circumstances a greater variation than usual must be allowed between the positions of the faces as determined by measurement and by calculation.

The new forms are characterized as follows:

a $= +\frac{1}{3}$ **0 (103)** observed on crystal No. 8 with one face,

measured: $\phi\rho = 269° 14$; $71° 35$
calculated: $\phi\rho = 270° 00$; $71° 34$

The form is a faint or weak one, but certain.

g' $= -\frac{2}{5}$ **0 ($\bar{2}$05)** observed on crystal No. 1 with one small face. On account of the minute size of the face the signal reflection was somewhat extended in the zone c ð e; however, it was possible to fix the position. This gave:

Distance from pole, ρ in zone c ð e $= 21° 36$
calculated, $= 21° 48$

b' $= -\frac{2}{3}$ **0 ($\bar{2}$03)** observed on crystal No. 1 in the same zonal part as g'. The signal-reflection of the exceedingly small face gave in the zone c ð e the distance from pole $35° 22$ (calculated $= 33° 41$). This would not suffice to establish the form. However the form occurs with a better face on the same crystal. This gives a good signal reflection showing the position:

measured: $\phi\rho = 89° 27$; $56° 08$
calculated: $\phi\rho = 90° 00$; $56° 18$

b' occurs also on crystal No. 4, with one face having the position:

measured: $\phi\rho = 1° 43$; $34° 31$
calculated: $\phi\rho = 0° 00$; $33° 41$

The form may be considered as established. The simultaneous occurrence of g' and b' in the same zonal part corroborates the symbol from the fact that the law of the number series is

complied with; that is, the numbers form a legitimate series. We have the following discussion of the numbers:*

Letters:		c	g·	b·	ȣ	e
Symbol	pq	0	$\frac{3}{5}0$	$\frac{3}{3}0$	10	20
p	=	0	$\frac{2}{5}$	$\frac{2}{3}$	1	2
$\frac{p}{2}$	=	0	$\frac{1}{5}$	$\frac{1}{3}$	$\frac{1}{2}$	1
$\frac{v\dagger}{1-v}$	=	0	$\frac{1}{4}$	$\frac{1}{2}$	1	∞
2v	=	0	$\frac{1}{2}$	1	2	∞ = normal series N2.

h = + $\frac{3}{5}$ **0 (305)** observed on the peculiar crystal No. 6 (fig. 5), along with the form c = 0. The faces are very narrow and in consequence of this the signal reflections are somewhat extended in the zone; still the form may be considered as established since it occurs on all the edges, and the middle position corresponds to that obtained by calculation. Confusion of this form with the neighboring one e = + $\frac{1}{2}$ 0 is not possible and, moreover, e occurs beside h on two edges. For determining the symbol the angle formed with the adjoining cubic face suffices. This gave for the nine faces capable of being measured the following values:

h : c = 30° 33; 30° 35; 30° 28; 31° 02; 31° 37; 31° 39; 31° 58; 30° 29; 30° 53

Average, measured = 30° 53
calculated = 30° 58

This crystal is peculiar in that the form p, the octahedron, is entirely wanting and the form e as compared with h is unimportant.

k = $\frac{1}{4}$ **(114)** observed on crystal No. 5 with two small faces, the better of which gave the position:

measured: $\phi\rho$ = 13° 46; 76° 07
calculated: $\phi\rho$ = 14° 02; 76° 22

This form occurs again on crystal No. 7 with one face, which is uncertain, however.

m = $\frac{1}{3}$ **(113)** observed on crystal No. 1 with two small faces in the zone c p, the better of which gave the position c m =

* Concerning such a discussion of numbers compare Zeitschr. f. Kryst., xxviii, 24, 1897.

† v indicates always the member of the previous series which is to be transposed to the corresponding one of the following series.

25° 05, calculated c m = 25° 14. The same form was observed on crystal No. 4, with three faces, with the positions:

Measured.		Calculated.	
ϕ	ρ	ϕ	ρ
133° 18	24° 57	135° 00	25° 14
162° 33	73° 12	161° 34	72° 27
$\bar{4}5°$ 05	25° 26	$\bar{4}5°$ 00	25° 14

This form may be considered as established.

q = ½ **(1$\bar{1}$2)**, after c p e the most important form of *sperrylite*. It was observed on crystal No. 4 with four faces. The best of these gave the position:

measured: $\phi\rho$ = 44° 36; 35° 12
calculated: $\phi\rho$ = 45° 00; 35° 16

also on crystal No. 1 this form was observed with two uncertain faces, and on crystal No. 5 with five faint faces, the two best of which gave the position:

measured: $\phi\rho$ = 135° 32; 34° 11	measured: $\phi\rho$ = $\bar{4}$4° 30; 34° 08
calculated: $\phi\rho$ = 135° 00; 35° 16	calculated: $\phi\rho$ = $\bar{4}$5° 00; 35° 16

On crystal No. 7 this form appeared with four faint faces and on crystal No. 3 with three faces, the best of which gave the position:

measured: $\phi\rho$ = $\bar{4}$4° 36; 35° 12
calculated: $\phi\rho$ = $\bar{4}$5° 00; 35° 16

It appeared again on crystal No. 9 with four small faces with extended signal reflection.

B = ⅗ **(335)** observed on several crystals, established, however, only by two faces, one on crystal No. 3 and another small, though good face, on crystal No. 4.

Crystal No.	Measured.		Calculated.	
	ϕ	ρ	ϕ	ρ
3	$\bar{1}$33° 30	39° 56	$\bar{1}$35° 00	40° 19
4	135° 07	39° 42	135° 00	40° 19

It is a poorly developed form.

u = **1½ (212)** observed on crystal No. 1 with one face, on crystal No. 4 with two faces, and on crystal No. 5 with seven faces. Position:

Crystal No.	Measured. ϕ	ρ	Calculated. ϕ	ρ
1	46° 46	67° 50	45° 00	70° 31
4	43° 30	68° 39	$\bar{4}5°$ 00	"
	44° 37	70° 03	45° 00	"
	135° 32	69° 58	135° 00	"
	$\bar{1}35°$ 35	67° 57	$\bar{1}35°$ 00	"
5	$\bar{2}7°$ 17	48° 06	$\bar{2}6°$ 34	48° 11
	28° 32	48° 31	26° 34	"
	46° 29	70° 23	45° 00	70° 31

The faces are very small, but their frequent occurrence gives them some importance.

$\psi = +\frac{11}{24}$ **(214)**, a form important for *pyrite*, occurs on crystal No. 4 with three faces, and on crystal No. 5 with two faces. The faces are very small but possess brilliant lustre.

Position:

Crystal No.	Measured. ϕ	ρ	Calculated. ϕ	ρ
	$\bar{1}17°$ 02	79° 45	$\bar{1}16°$ 34	77° 23
4	$\bar{6}5°$ 19	78° 16	63° 26	"
	167° 16	66° 10	165° 58	64° 07
5	75° 18	65° 05	75° 58	"
	152° 52	29° 00	153° 26	29° 12

$\mathbf{x} = -\frac{21}{33}$ **(2̄13)** observed on crystal No. 4 with one well bordered face giving a good signal reflection. Position:

measured: $\phi\rho = \bar{1}47°\ 05';\ 74°\ 57'$
calculated: $\phi\rho = \bar{1}46°\ 19';\ 74°\ 30'$

The agreement is satisfactory. The form may be considered as established.

? $\mathbf{z} = +\frac{31}{55}$ **(315)** observed on crystal No. 5 with one triangular face. Position:

measured: $\phi\rho = 48°\ 50';\ 79°\ 27'$
calculated: $\phi\rho = 49°\ 02';\ 80°\ 16'$

Owing to this agreement the form is a probable one, but confirmation is to be desired. Till that is obtained the form is indicated by a (?).

? $\mathbf{D} = +\frac{11}{23}$ **(326)** observed on crystal No. 7 with one face. Measurement and calculation do not agree very well. The form needs confirmation. Position:

measured: $\phi\rho = 159°\ 10';\ 64°\ 39'$
calculated: $\phi\rho = 161°\ 34';\ 64°\ 37'$

TABLE OF ANGLES.*

No.	Letter.	Symb.	Miller.	ϕ	ρ	ξ_o	η_o	ξ	η	X (Prisms) (x : y)	y	d $= \mathrm{tg}\,\rho$
1	c	0	001	—	0°00	0°00	0°00	0°00	0°00	0	0	0
		0∞	010	0°00	90 00	"	90 00	"	90 00	"	∞	∞
2	a	01	013	"	18 26	"	18 26	"	18 26	"	0·3333	0·3333
		03	031	"	71 34	"	71 34	"	71 34	"	3·0000	3·0000
		$\infty 3$	130	18 26	90 00	90 00	90 00	18 26	"	0·3333	∞	∞
3	g	$0\frac{3}{2}$	025	0 00	21 48	0 00	21 48	0 00	21 48	0	0·4000	0·4000
		$0\frac{3}{5}$	052	"	68 12	"	68 12	"	68 12	"	2·5000	2·5000
		$\infty\frac{5}{2}$	250	21 48	90 00	90 00	90 00	21 48	"	0·4000	∞	∞
4	e	$0\frac{1}{2}$	012	0 00	26 34	0 00	26 34	0 00	26 34	0	0·5000	0·5000
		02	021	"	63 26	"	63 26	"	63 26	"	2·0000	2·0000
		$\infty 2$	120	26 34	90 00	90 00	90 00	26 34	"	0·5000	∞	∞
5	h	$0\frac{5}{2}$	035	0 00	30 58	0 00	30 58	0 00	30 58	0	0·6000	0·6000
		$0\frac{5}{3}$	053	"	59 02	"	59 02	"	59 02	"	1·6667	1·6667
		$\infty\frac{5}{3}$	350	30 58	90 00	90 00	90 00	30 58	"	0·6000	∞	∞
6	b	0	023	0 00	33 41·	0 00	33 41·	0 00	33 41·	0	0·6667	0·6667
		0	032	"	56 18·	"	56 18·	"	56 18·	"	1·5000	1·5000
		$\infty\frac{3}{2}$	230	33 41·	90 00	90 00	90 00	33 41·	"	0·6667	∞	∞
7	δ	01	011	0 00	45 00	0 00	45 00	0 00	45 00	0	1·0000	1.0000
		∞	110	45 00	90 00	90 00	90 00	45 00	"	1·0000	∞	∞
8	k	$\frac{1}{4}$	114	"	19 28	14 02	14 02	13 38	13 38	0·2500	0·2500	0·3535
		14	141	14 02	76 22	45 00	75 58	"	70 32	1·0000	4·0000	4·1231
9	m	$\frac{1}{3}$	113	45 00	25 14·	18 26	18 26	17 33	17 33	0·3333	0·3333	0·4714
		13	131	18 26	72 27	45 00	71 34	"	64 45·	1·0000	3·0000	3·1623
10	q	$\frac{1}{2}$	112	45 00	35 16	26 34	26 34	24 05·	24 05·	0·5000	0·5000	0·7071
		12	121	26 34	65 54·	45 00	63 26	"	54 44	1·0000	2·0000	2·2360
11	B	$\frac{3}{5}$	335	45 00	40 19	30 58	30 58	27 13·	27 13·	0·6000	0·6000	0·8485
		$1\frac{3}{5}$	353	30 58	62 46·	45 00	59 02	"	49 41	1·0000	1·6667	1·9437
12	p	1	111	45 00	54 44	45 00	45 00	35 16	35 16	1·0000	1·0000	1·4142
13	u	$\frac{1}{1}$	122	26 34	48 11·	26 34	"	19 28	41 48·	0·5000	1·0000	1·1180
		2	221	45 00	70 31·	63 26	63 26	41 48·	"	2·0000	2·0000	2·8284
14	ψ	$\frac{1}{2}$	124	26 34	29 12·	14 02	26 34	12 36·	25 52·	0·2500	0·5000	0·5590
		$\frac{1}{2}$	142	14 02	64 07·	26 34	63 26	"	60 47·	0·5000	2·0000	2·0615
		24	241	26 34	77 23·	63 26	75 58·	25 52·	"	2.0000	4·0000	4·4721
15	?D	$\frac{1}{3}$	236	33 41·	31 00	18 26	26 34	16 36	25 22·	0·3333	0·5000	0·6009
		$\frac{2}{3}$	263	18 26	64 37·	33 41·	63 26	"	59 00	0·6667	2·0000	2·1130
		$\frac{3}{3}$	362	26 34	73 24	56 18·	71 34	25 22·	"	1·5000	3·0000	3·3541
		$\frac{1}{3}$	123	"	36 42	18 26	33 41·	15 30	32 18·	0·3333	0·6667	0·7453
16	x	$\frac{2}{3}$	132	18 26	57 41·	26 34	56 18·	"	53 18	0·5000	1·5000	1·5811
		$\frac{3}{3}$	231	33 41·	74 30	63 26	71 34	32 18·	"	2·0000	3·0000	3·6055
17	?z	$\frac{1}{5}$	135	18 26	32 18·	11 18·	30 58	9 44	30 28	0·2000	0·6000	0·6325
		$\frac{1}{3}$	153	11 18·	59 32	18 26	59 02	"	57 41·	0·3333	1·6667	1·6996
		35	351	30 58	80 16	71 34	78 41·	30 28	"	3·0000	5·0000	5·8310

* In the table of angles the ± forms are not separated.

Heidelberg, Germany, and
School of Mining, Kingston, Canada.

Art. XLVI.—*The Niagara Domes of Northern Indiana;* by Edward M. Kindle.

The earlier workers on the geology of Indiana generally believed the geological structure of the State to be much simpler than recent investigations have shown it to be. Their firmly-rooted belief in the uniformity of the geological structure of Indiana prevented many of them from even entertaining any explanation of the Niagara dips which would include deformation. Prof. John Collett, for many years State Geologist, says in describing Niagara beds which show dips: . . . "the uniform undisturbed condition of the strata of this and adjoining states will not allow the presumption of upheaval and such dislocation of strata as would account for these phenomena."* Starting with the assumption of the "undisturbed condition" of the strata, it is not surprising that Collett was compelled to confess, in describing a locality exhibiting tilted beds, that, "This quarry is a mystery."†

It will be the purpose of the following pages to show that we have in the Niagara beds of northern Indiana a notable exception to the nearly horizontal and undisturbed condition which generally characterizes most of the other formations in Indiana. A number of theories concerning the structure of the Niagara area and the dips have been advanced, and the more important ones will be briefly reviewed.

Owen.—Richard Owen first directed attention to the tilted Niagara rocks of northern Indiana. Forty years ago Owen wrote that he had found near Delphi, "A local dip to the southeast amounting to 40°."‡ He mentioned also the sharp dips at Wabash and Huntington.

Cox.—Ten years later E. T. Cox, then State Geologist, gave a brief description of these localities, but ascribed the inclined beds to false bedding.§

Oblique or false bedding on a large scale is an unusual phenomenon in limestones. The seas in which they are deposited are seldom sufficiently shallow to permit the action of currents strong enough to produce oblique bedding. As a possible explanation of the dips, however, the hypothesis of oblique bedding proposed by Cox deserves an examination. The history of the discussions concerning the LeClaire limestone of Iowa illustrates the difficulty sometimes met with in discovering positive evidence as to the character of bedding.

*12th Ann. Rep. Ind. Geol. Surv., p. 59. †L. c.
‡Ind. Geol. Surv., 1859–60, p. 98, 1862.
§Ind. Geol. Surv., 1872, p. 307.

Even where true bedding seems obvious it is not always easy to discover in the strata intrinsic evidence which will entirely eliminate the possibility of false bedding. At a number of localities highly inclined beds richly fossiliferous have been observed in contact with barren or nearly barren beds where the evidence of tilting seemed so clear as to require no special comment but for the fact that the opposite opinion has been held regarding them. The final test of the false bedding

FIG. 1.—Tilted beds near Kentland.

hypothesis comes in applying it to particular cases. Does it afford a satisfactory explanation of such examples as the following? Near Delphi a bed fifteen feet or more in thickness occurs, composed almost entirely of the shells of *Conchidium laqueatum* one to three inches in length, which is inclined at an angle of 20°. While it is perhaps not impossible that such a bed should occur in false bedded strata, it appears to the writer highly improbable. In the case, however, of strata having a dip of 70° or more, as shown in fig. 1, oblique bedding cannot be considered even as a possible explanation. The materials composing the beds, if piled in layers inclined at such a high angle, could not retain their position. Deformation alone offers a satisfactory explanation of such dips.

Gorby.—In 1886 S. S. Gorby* described a considerable number of outcrops showing tilted strata, and announced that they indicated a great anticlinal, "extending entirely across the State," which he called the "Wabash arch." Many of the dips recorded by the author of this hypothetical arch afford evidence against it. About half of them are east or west dips, while the supposed arch has a southeast and northwest axis, which calls for northeasterly and southwesterly dips, and fails to explain the others. Phinney in discussing the "Wabash arch" points out that the gas well records furnish evidence against it.

Phinney.—The tilted strata are, according to Phinney, due to irregularity of deposition rather than to deformation, and he states that they are "to be attributed to the building up of coral reefs and rocky prominences over portions corresponding to the domes and offshoots of the Cincinnati arch or to irregularities in the sea-bottom."† A serious objection to the coral reef theory is the absence of the reefs. Corals are not at all abundant in the Niagara of northern Indiana. They have not been observed anywhere in sufficient abundance to form reefs. While inequalities in the sea-bottom may be responsible for some of the smaller undulations in the strata, neither they nor "offshoots of the Cincinnati arch" appear to offer a satisfactory explanation of dips of 45° to 75° in the Niagara rocks, which the accompanying photographs show.

Thompson.—Maurice Thompson considered the dips to be the result of the disturbance of beds originally horizontal. He states: "The structure of the Niagara limestone does not in the least indicate false bedding."‡ Thompson accepted Gorby's hypothetical "Wabash arch," but evidently had a pretty clear understanding of the local dome-like structures in the Niagara. A summary of his views is best stated in his own words: "The arch formed by this upheaval consists of a vast series of low bubbles or cones that make the surface of the Niagara limestone somewhat like that of a sea in a brisk breeze."§

Elrod and Benedict.—In the same volume with Thompson's paper appeared a paper by M. N. Elrod and A. C. Benedict on a portion of the northern Indiana Niagara area. These authors conclude that the Niagara "cones" which they describe are the result of a variety of cleavage which simulates stratification, and cuts across the original bedding planes.

* 15th Ann. Rep. State Geol. of Ind., 1886, p. 241.
† 11th Ann. Rep. U. S. Geol. Surv., p. 653.
‡ 17th Ann. Rep. Ind. Geol. Surv., 1891, p. 183.
§ Ibid., p. 185.

A second paper by the same authors appeared in 1894 in which they deny that there has been any tilting of the Niagara beds of the Wabash Valley.* Cleavage or a "modified form of joint structure" is the explanation offered of the dips. This hypothesis would deserve careful consideration but for the testimony of the fossils. The occurrence of richly fossiliferous bands running parallel with the so-called cleavage planes affords conclusive evidence that they are true bedding planes.

Kindle.—During the summer of '99 the writer visited a number of outcrops in northern Indiana and became convinced that the dips of the Niagara outcrops in northern Indiana were not apparent but real dips. Two of the outcrops showing tilted strata were described in a recent paper.†

Distribution and Composition of Beds.—The area in which tilted rocks occur extends from the Illinois line in Newton County eastward nearly to the Ohio line, embracing the upper Wabash Valley. Highly tilted beds occur in Hamilton County, seventy miles south of the Wabash. It is probable that the Niagara rocks in the intervening territory have heavy dips at many localities, but the thickness of the drift over this area makes it impossible to ascertain whether or not this is true.

The Niagara rocks of this region consist principally of magnesian limestones varying in texture from the very hard close-grained gray limestone breaking with conchoidal fracture to a soft, easily crumbling buff limestone. Beds of buffish or drab calcareous shale are sometimes associated with the limestone. Local lenses of sandstone have been observed in the Niagara limestone at some localities. One of these near Kenneth has a thickness of five and one-half feet, and is composed of pure white quartz sand containing only a trace of calcium carbonate and iron. The Niagara outcrops show only a small portion of the total thickness of the formation, none of the sections extending down to the Lower Silurian. The information which we have concerning its thickness is derived from well records. From these it appears that the Niagara has a thickness of from 250′ to 500′. It does not appear practicable at present to divide these beds into two or more divisions, although detailed study of the paleontological material which has been collected may indicate the desirability of such a division.

There are local facies with characters peculiar to themselves, but not persistent for any considerable distance. A good example of this is seen at the quarries at Decatur. The lime-

* 19th Ann. Rep. State Geol. of Ind., p. 22.
† This Journal (4), xiv, pp. 221–224.

stone in the quarry at the north side of the town is a soft saccharoidal, nearly pure white stone, while at the Bowers quarry, one-third of a mile to the northwest, the rock at apparently the same horizon is a very hard gray dolomitic limestone.

The paleontological data for the correlation of the beds under discussion with the Niagara of New York was presented in a previous paper* by the writer. The beds appearing at the localities then described are identical in faunal and physical characters with those occurring elsewhere in the northern Indiana area. It appears therefore desirable to continue to use the term Niagara, which has long been applied to the dolomites of northern Indiana.

Structure.—The general structure of the Niagara of northern Indiana is that of a broad arch with gently sloping sides trending northwest and southeast. It represents a northwestern extension of the Cincinnati geanticline. Its axis, approximately located, enters the State near Richmond, and passes northwesterly in the vicinity of Muncie, Marion, and Peru, and continues north of the Wabash through Cass, White, Jasper, and Newton counties into Illinois. On the two sides of this line of maximum elevation of the Niagara, the Devonian and Carboniferous rocks dip in opposite directions; in Michigan and Ohio, toward the north and northeast; in Indiana, toward the southwest or south. The following table shows the elevation of the top of the Niagara A. T. along a line approximately at right angles to this axis, extending from Crawfordsville to Auburn, Indiana, a distance of 140 miles.

Crawfordsville	81 A. T.
Frankfort	463
Kokomo	778
Wabash	652
Columbia City	599
Auburn	50

The arch described above is not the "Wabash arch" of Gorby, which apparently was supposed by its author to follow the Wabash Valley in eastern Indiana. The tilted beds which were cited as the evidence of the "Wabash arch" will be shown in another part of this paper to be independent of the above described arch in origin.

The figures given above are based upon gas well records published by the Indiana Geological Survey,† in the northern Indiana area.

* This Journal (4), xiv, pp. 221–224.
† 16th Ann. Rep. Ind. Geol. Surv., pp. 217–218.

Dips.—A large percentage of the outcrops throughout the Niagara area are characterized by dips of from 5° to 80°. In a few instances the dips represent cleavage planes. In one case (at the Means Quarry, Newton County) both cleavage and bedding planes are well defined and nearly at right angles to each other, both being inclined at a high angle to the horizontal. A few localities have also been noted where the dip is the result of irregular or false bedding. But the great majority of the dips can be referred to neither of these causes. They are clearly the result of the deformation of strata which were originally horizontal.

A brief study of the tilted beds will suffice to show that they are not referable to ordinary anticlines. A description of the beds near Kentland will illustrate this. At the Means Quarry (N.E. of N.W. of sec. 25) the rock is a hard gray limestone very fine-grained, in strata 3″ to 25″ in thickness, dipping N. 60° to 65°, with strike of N. 82 E. At the McKee quarry, less than half a mile to the east, the strata dip 70° to 75° toward the east with a strike of N. 12° W., or nearly at right angles to the strike at the Means quarry.

Numerous exposures of the Niagara limestone occur near Delphi which show dips of from 10° to 45°. The dips here, like those at Kentland, do not conform to an anticlinal structure but appear to be quaquaversal.

Domes.—At Wabash a fine exposure of the Niagara occurs near the railroad station which affords a key to the puzzling dips at Kentland and other points where only small exposures can be seen. A railroad cut has exposed a cross section through the center of a small dome in the Niagara limestone and shale. In passing through the cut the beds are seen dipping in all directions from the center. On the northeast, east, and south of the dome the Wabash River has denuded the dome of superficial deposits and the beds are seen dipping sharply in these several directions, as shown in the accompanying photographs. At the south end of the cut the strata dip 25° to 30° S. 40 W. Toward the north end of the cut they are seen dipping to the northwest and north. On the east side the dips are east and southeast. The width of the dome from north to south exposed in the cut is about 840′. A portion of the south side of the dome has been removed by erosion. It is estimated that the total north and south diameter has been about 1150′. The excavation for the railroad gives a continuous exposure from the center of the dome of the Niagara shale and limestone for half a mile. The dips of the beds going north from the center of the dome are observed to drop down gradually from a maximum of 30° to perfectly horizontal beds. No dips or other disturbances are noticeable in

2

FIG. 2.—N. E. part of Wabash dome.

3

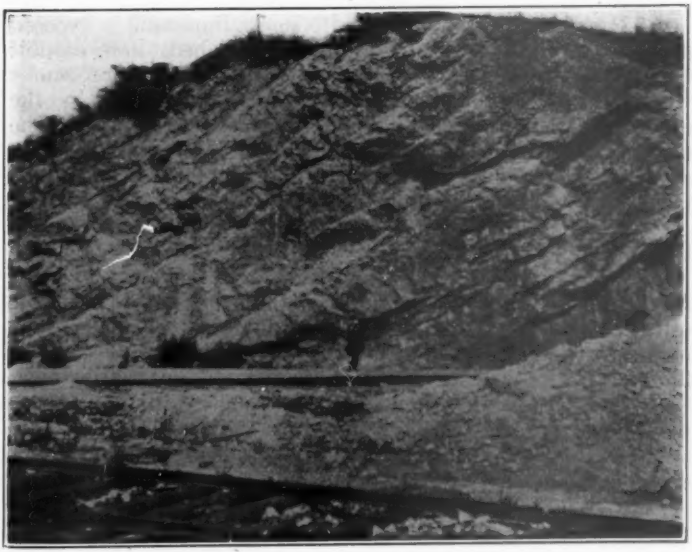


FIG. 3.—S. W. part of Wabash dome.

the half mile exposure, north of the dome. "Slickened-sides" characterize the contact of some of the beds in the dome.

A group of small domes occurs in the Wabash Valley near Lagro. One of these, known as the Hanging Rock, is situated about one mile southeast of Lagro. It consists of a mass of limestone and shale which rises abruptly from the bank of the Wabash River to a height of about eighty feet. The beds exhibit quaquaversal dips and represent a part of a dome, the north and east sides of which have been cut away by the river. The upper beds dip to the south and west at an angle of 25°. The lower strata are less inclined.

4

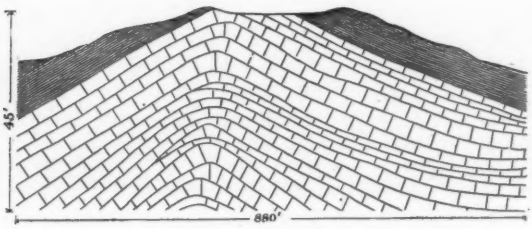


FIG. 4.—Cross section of Wabash dome.

At Huntington, a large area of Niagara limestone is exposed at the quarry one mile east of town. The beds here exhibit quaquaversal dips and indicate a dome having its center within and near the east side of the area exposed by the quarry. The highest dips noted here are 35° or 40°. The exposures are not sufficiently extended to determine the exact diameter of the dome, but from the horizontal strata exposed in nearby outcrops it is probable that it does not exceed 2000′. At the old quarry, a quarter of a mile northeast, the strata are horizontal.

The quaquaversal dips show a general tendency to drop rapidly from a high inclination to nothing. This is noticeable at the quarry half a mile west of Huntington. The beds in the new quarries on the north side of the railroad show a dip of 25° to the south, while the strata in the old quarry not more than 100 yards away show dips of from 1½° to 0. It was doubtless the proximity of outcrops showing highly tilted strata to those of horizontal beds which led Owen* into the error of reporting tilted Devonian rocks at Huntington resting

*Ind. Geol. Surv., 1859-60, p. 66.

unconformably on horizontal Niagara beds. Neither Devonian beds nor unconformity exist in the vicinity of Huntington.

It has been shown that the dips observed in at least three localities are quaquaversal and the expression of dome structure. Nowhere in the area does the strike and dip of the beds conform to an anticlinal or synclinal structure. The dips seem everywhere to be quaquaversal and it is believed that all of the tilted Niagara beds of northern Indiana represent small domes similar to those at Huntington.

Origin and Age.—There is at present no positive evidence as to the nature of the forces which produced the domes. It seems probable, however, that they may be analogous in origin to the "mud lumps" at the mouth of the Mississippi. From a recently published description of the "mud lumps" they appear to have a similar structure to the Niagara domes. Harris has studied these interesting masses of recently elevated sea-bottom and states, "That they rise up in domes or anticlinals and preserve their regular bedding is proven by their present structure." *

Whatever the causes may have been which produced the domes, there is clear evidence that they were developed about the close of the Niagara period. Many of them were elevated above the Palaezoic sea, while others probably did not reach its surface. Some of the domes remained above sea-level during a considerable portion of the Devonian age, and there is some evidence that others continued as islands to the end of Devonian time. The evidence of the exposures indicate that the gradual deepening of the Devonian sea which initiated the "Black shale" sedimentation submerged some of the Niagara domes which had remained above sea-level throughout the Corniferous and Hamilton epochs. These conclusions are based upon the relations which have been observed to exist between the Devonian and Niagara beds at Delphi, Georgetown, Kentland, and other localities. At Georgetown, the horizontal Corniferous limestone beds rest on Niagara beds showing a small dip, but there is no evidence of land surface conditions having existed previous to the Corniferous sedimentation at that locality.

Near Delphi, at one locality, horizontal beds of Hamilton age lie unconformably above the Niagara limestone, which shows a dip of 45°; a bed of ferruginous clay and iron ore separate the two. The New Albany shale is seen resting on the Niagara at another locality near the one last mentioned, the ferruginous clay separating these two as in the first instance.

* Geol. Surv. of La., 1902, p. 39.

The highly tilted beds near Kentland occur on high ground in the open prairie remote from any stream. Rocks of later age have been encountered at much lower levels in all directions from this point within two or three miles, and it appears probable that the Kentland dome remained above sea-level until the end of the Devonian or later.

The occurrence of outliers of Mansfield sandstone (Pottsville conglomerate) in the center of the Niagara area of northwestern Indiana near Remington and Jasper indicate that a subsidence occurred after the formation of the Niagara domes in northwestern Indiana which submerged all or nearly all of the Niagara area of that region beneath the Carboniferous sea. The development of the present Niagara arch in northwestern Indiana was therefore of much later date and independent of the formation of the Niagara domes. While the domes date back to the end of the Niagara, the Niagara arch is of Carboniferous or post-Carboniferous age.

The Niagara-Devonian unconformity which has been described, though much more pronounced, may be correlated with that which has been recognized in Shelby County,* Indiana, and with the slight unconformity between the Devonian and Upper Silurian which has been recorded by Newberry,† on the east side of the Cincinnati geanticline. The evidence at hand points to a general elevation of the sea-bottom at the close of the Niagara in the area around the northern end of the Cincinnati geanticline. The resulting shallow sea was doubtless an important factor in the sudden disappearance of the Niagara fauna and the substitution of the coralline fauna of the Corniferous.

U. S. Geological Survey, New Haven, Ct.

* 25th Ann. Rep. Ind. Geol. Surv.
† Geol. Surv. of Ohio, vol. i, p. 106.

ART. XLVII.—*A New (?) Meteoric Iron from Augusta Co., Virginia;* by H. D. CAMPBELL and JAS. LEWIS HOWE.

IN the list of accessions to the mineral collection of Washington and Lee University during the session 1870–71 occurs the following note: "From Wm. N. Wilson, Esq., Augusta County, Va., a fine specimen of meteoric iron." There is no doubt but that the above entry refers to the meteorite now under consideration, inasmuch as it is the only one in the University collection which has no label, and there is no meteorite with the foregoing label. Since 1880 the history of this meteorite is definitely known. From 1886 to 1894 the meteorite was in charge of Professor W. G. Brown. A fragment was cut from one end by him for analysis, the surface etched and photographed but Professor Brown's analysis has not been published.

The question has naturally arisen as to whether this meteorite is from the same fall as the so-called Staunton meteorite which has been several times analyzed and in which Sir William Ramsay found the presence of helium. The meteorite was sent to Professor Henry A. Ward to be again cut and an analysis of it was made for him by Mr. J. E. Whitfield. A fragment was also sent to Dr. Ramsay and examined by him for gases.

The analysis by Whitfield is given together with the other analyses of the Augusta meteorite for comparison, and also the result of the examination by Dr. Ramsay.

	1	2	3	4	5	6	6*a*	7	7*a*
Iron	88·706	88·365	89·007	91·439	90·293	92·49	93·27	89·850	91·376
Nickel	10·163	10·242	9·964	7·559	8·848	6·38	6·04	7·560	7·689
Cobalt	·396	·428	·387	·608	·486	·63	·64	·600	·610
Copper	·603	·004	·003	·021	·016	·0258	...	·065	·066
Tin	·002	·002	·003	trace	·005	...	...	...	...
Phosphorus	·341	·262	·375	·068	·243	·24	...	·158	·161
Sulphur	·019	·008	·026	·018	·012	...	...	·006	·006
Chlorine	·003	·002	·004	trace	trace	...	...	...	...
Carbon	·172	·185	·122	·142	·177	·05	·05	·046	·047
Silicon	·067	·061	·056	·108	·092	...	...	·045	·045
Oxide	...	...	...	...	...	...	...	1·560	...

1. J. W. Mallet, this Journal (3), ii, 10. Mass weighing 56 pounds. 1871.
2. J. W. Mallet, ibid. Mass weighing 36 pounds. 1871.
3. " ibid. Mass weighing 3·5 pounds. 1871.
4. " this Journal (3), xv, 338, 1878.
5. Kunz, this Journal (3), xxxiii, 58, 1887.
6. Cohen, Ann. Hof. Mus., vii, 156, 1892.
6*a*. " ibid. Calculated from 6, less 1·56% schreibersite.
7. Whitfield. Calculated from 7*a*, less oxidized portion. 1901.
7*a*. "

Regarding the gases in the meteorite we are permitted to quote the following from Dr. Ramsay:

"The gases were extracted by heating *in vacuo*. This gas is very curious. There were only 3·52^{cc}, none of it dissolved in KOH, Aq. On mixing with oxygen and sparking, there was contraction, and afterwards a large absorption of CO_2 with potash. The residue was very minute, so small indeed that on letting it into a small exhausted tube it was at phosphorescent stage. But with a jar and spark-gap it was possible to see the argon blues, and without the jar the argon reds were just visible. I think I saw the helium yellow, but it was very feeble. The complete analysis is as follows:

Volume of gas from 6·54 grams of the meteorite,

	3·52^{cc}		
KOH. No contraction.			
Oxygen added	9·18		
	12·70		
After sparking	12·00		
Contraction for H_2O	0·7	×2/3	0·46 H_2
Absorption with KOH	8·83		
CO_2 formed	3·17		3·17 CH_4
Add	0·46		
	3·63		
Argon, etc., say	0·02		0·02 A, etc.
	3·65		3·65 "

Conceivably there may have been a trace of ethane, or of some hydrocarbon richer in carbon, in which case the CO_2 would not have been equal to the CH_4, but greater in volume. This might account for the small discrepancy between the amount taken, 3·52^{cc}, and the total, 3·65."

The question as to whether this meteorite is a portion of the "Staunton" fall is not settled by the analysis. It differs somewhat from the specimens analyzed by Mallet in 1871, but on the other hand it resembles in most particulars that analyzed by Mallet in 1878. It should be noted that Brezina considers that this latter specimen is not from the same fall as those earlier analyzed. The analysis of the gases would seem to point toward the present meteorite being identical with the "Staunton," though it does not decide the question. It is peculiar in containing chiefly methane, but like the "Staunton" contains argon. "Staunton" is the only meteorite reported in literature, so far as we have been able to find, as containing

helium, and in this there was but a trace. In the case of the present meteorite the presence of helium seems probable but not certain.

As regards the etched surfaces, they do not resemble those of the "Staunton." Mr. H. L. Preston has called our attention to the complete absence of the club-shaped "kamacite" blades, so prominent in the "Staunton." These are also lacking in one other mass of the "Staunton," and it was chiefly from this consideration that Brezina held that one to be a distinct fall.

Mr. Wirt Tassin of the United States National Museum has had the kindness to compare the photographs of the etched surfaces with the section of the "Staunton" iron described by Mallet in 1871, and writes as follows: "It shows quite a difference in structure. The 'taenite' plates in the museum specimen are smaller, averaging only half the size of those in the photograph. The specimen also shows numerous fine lines of 'schreibersite,' often regularly arranged—especially in the 'plessite,' and are occasionally so abundant as to give to it (the 'plessite') a stippled appearance. These are lacking in the photograph, although this may be due to differences in illumination. Finally, as you have already remarked, there is the complete absence of the bulb- or club-like 'kamacite' blades."

We propose to call this meteorite "Staunton, No. 7." The original weight of the meteorite was somewhat more than seven kilos; its weight prior to the recent cutting was 7·15 kilos; its present weight is 6·04 kilos.

Washington and Lee University, Lexington, Va.

Art. XLVIII.—*The Diffusion of Vapor into Nucleated Air;** by C. Barus.

1. The apparatus with which experiments of the present kind are made is conveniently described by aid of the accompanying diagram. The appurtenances necessary in practice are given in my report on the structure of the nucleus (Smithsonian Contributions, No. 1373, 1892), to which reference will frequently be made. A is a tall glass vessel about one meter high, either cylindrical or rectangular in section, in the latter case with opposed plate glass sides. The liquid, L, whose vapors are to be tested, is placed in the bottom. The wide tube, c, is used for sudden exhaustion, while a vacuum gauge, g, registers the pressure differences. The tubes a and b to the *top* and the *bottom* of A serve for the admission either of filtered air or of nucleated air. They are used together, one for influx and the other for efflux, in connection with the suction of an aspirator.

When the diffusion of the necessarily heavy vapors from L is to be measured, the air in A is first cleansed of vapor by a current of nucleated air from a to b. Thereafter the stopcocks are closed at a stated time. If now at a subsequent time a sudden exhaustion is made in A through c, for a stated pressure difference $\delta p'$, shown at g, the progress of the diffusion may be computed from the height of the fog-bank after an allowance is made for the rise due to the exhaustion.

On the other hand, if the aspirating current is of *filtered* air and moves in the direction from b to a, over the surface of the *volatile* liquid, the receiver, A, should become uniformly saturated to a high degree throughout. If nuclei are added at a stated time below, near the surface of the liquid, the corresponding height of the fog-bank seen on exhaustion at a later time, should indicate the rate at which the nuclei diffuse, if they diffuse more slowly than the residual concentration of vapor. This method for nuclei, which I pursued with entire confidence, leads however to erroneous results, as the present paper will show: for the diffusion of the nuclei is a much more rapid process than the accompanying complications of vapor diffusion.

2. To state the case specifically, let p be the vapor pressure relative to the saturation pressure at the temperature ϑ_0, at a time t after diffusion of vapor commences and at a height x

*Paper contributed to the meeting of the National Academy, April, 1903.

above the surface of the liquid in the receiver, A. Then from well known principles it may be shown that

$$p = 1 - \frac{2}{\sqrt{\pi}} \int_0^{x/2\sqrt{kt}} \epsilon^{-q^2}\, dq \qquad (1)$$

where k is the coefficient of pressure diffusion.

If the exhaustion at the time t is made adiabatically from air pressure p'_0 to p' corresponding to the temperatures ϑ_0 and ϑ', the relation is approximately $\vartheta'/\vartheta_0 = (p'/p'_0)^{(\gamma-1)/\gamma}$ where a correction for precipitated liquid, etc., is needed.

The vapor pressure corresponding to the reduced temperature ϑ' so obtained after division by the saturation pressure at ϑ_0, is then the value of p in equation (1), which therefore like x and t is known so that k may be computed.

3. In order to have an example for use in the discussion below, I computed the case for water vapor, which though unsuitable from its lightness for experiment, is convenient for comparison with other vapors, almost all of which are heavier than air. The well known expansion of (1) judiciously manipulated is sufficient for the purpose, though I afterwards availed myself of the tables in Dienger's Method of Least Squares in the absence of larger tables.

TABLE I.—Pressure diffusion of water vapor into air. Initial ($t = 0$) saturation, $p_0 = 0$. $k = \cdot 23$ at 20°.

Time t	Values of p when height of fog-bank $x =$ 1cm	5cm	10cm	20cm	40cm	60cm
1m	·85	·34	·06	--	--	--
5	·93	·68	·40	·10	--	--
10	·94	·77	·56	·24	·02	--
20	·97	·83	·68	·40	·10	·01
40	·98	·88	·77	·56	·24	·08
60	·98	·91	·81	·63	·34	·15
100	·99	·92	·85	·71	·45	·26
	The same for the initial saturation, $p_0 = \cdot 33$.					
1m	·90	·56	·37	--	--	--
5	·95	·79	·60	·39	--	--
10	·97	·84	·71	·49	·34	--
20	·98	·89	·79	·60	·39	·34
40	·98	·92	·84	·71	·49	·38
60	·99	·94	·87	·75	·55	·43
100	·99	·95	·90	·81	·53	·50

The results for water vapor are given in Table I, where the time, t, is in minutes and the height of the fog-bank, x, is in centimeters.

The table also contains a second series of data, in which the diffusion takes place into a vapor $\frac{1}{3}$ saturated, to which reference will be made below. The results of Table I may be constructed graphically, showing respectively the advance of diffusion at a given height or at a given time. The latter are exhibited in connection with figure 1. From either set of curves the parabolas which show the rise of a given vapor pressure in the lapse of time may be obtained by graphic interpolation.

If the exhaustion chosen is such as to reduce the vapor pressure to $\frac{1}{3}$ the intersection of the vertical line of the figure with the successive curves will show the heights of the fog-banks on condensation. Thus after 20, 40, or 60 minutes the fog-banks having attained heights of roughly 23, 33, and 40 centims., will be in good position for observation.

For any other vapor than water, the times will increase inversely as the coefficients of diffusion. Thus for benzol § 4, the time intervals should be increased about $2\frac{1}{2}$ times; etc.

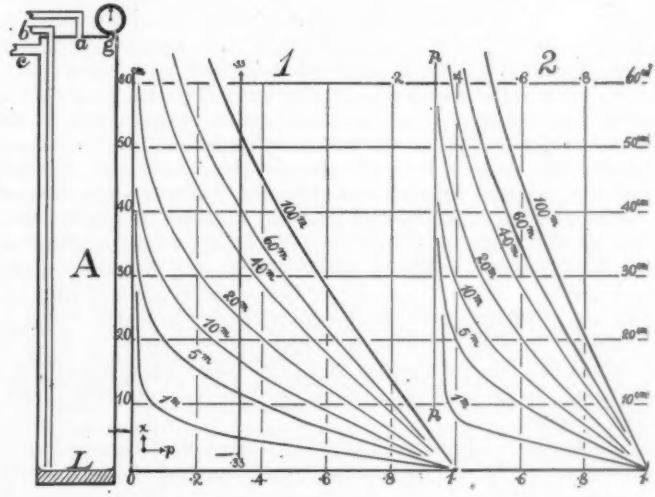
4. The feature of these curves is the extreme slowness of diffusion even for water vapor. At but 20^{cm} above the liquid surface it takes half an hour to reach semi-saturation. The case is accentuated for other liquids where the coefficients are smaller, as for instance for the following liquids at about 20° ;

Vapor,	CS_2	C_6H_6	H_2O	CH_4O	C_2H_6O	C_3H_8O
$k=$	·1	·09	·23	·16	·12	·07

If therefore the fog particles are relatively large and subside rapidly, the air will soon become highly desaturated. In other words, if the air in the receiver A is cleaned of nuclei by condensation, there is no vapor available to replace the liquid lost.

In case of water vapor the fog particles are small and subside slowly while the vapor is lighter than air. Hence the latter is liable to be reheated from the rapid radiation of gases assisted by convection as stated, before much desaturation takes place, unless the vessel is very long and the sides dry. The opposite is the case for the hydrocarbon vapors in spite of their volatility, since the fog particles are larger and fall rapidly and the vapors are heavier than air. After successive precipitation at a given pressure difference, the vapor may be so far desaturated that it nearly ceases to condense even if nuclei are present. It cannot quite cease to respond, for some vapor must return to the air after condensation almost instantaneously; but it is not improbable that a vapor exhausted to a slightly higher pressure difference will fail to condense thereafter at the original pressure difference. Thus there is considerable chance for error in judgment and what is taken for the diffusion of nuclei

added near the surface of the liquid may actually be the diffusion of the liquid itself. This will even be the case if an aspiration current, as in § 1, falls sufficiently short of saturation, supposing always that the velocity of nuclei is relatively large. True, in the experiments which I made the two sets of results for nuclei and for vapors differ in order of values, in distribution among different vapors, in forms of loci, while for carbon disulphide the gradually increasing apertures of the coronas is certain evidence of greater concentration of nuclei. But these and other occurrences may each in their turn be explained away.



5. To facilitate the discrimination in question the diffusion of vapor into partially saturated vapor may be computed as has already been done in Table I, where

$$p = 1 + \frac{2}{\sqrt{\pi}}(p_0 - 1)\int_0^{x/2\sqrt{kt}} \epsilon^{-q^2}\, dq$$

and the initial saturation is $p_0 = \frac{1}{3}$ at $t = 0$. The results of the table are constructed in figure 2 and from them the parabolas showing the rise of the levels of successively increasing saturation may be derived.

An inspection of figure 5 shows that if the exhaustion were carried somewhat further than corresponds to the lower limit $p = \frac{1}{8}$, the fog-banks would be capped at a definite height, and that the latter would be enormously influenced by slight

changes of pressure decrement on exhaustion. Experiments bears this out. Even for fixed air pressure differences ($\delta p'$), the condensation must progress with a sweep from the bottom upward, and if the very small particles last formed evaporate fast enough, an upper demarcation of the fog-bank will again show itself which would easily be mistaken as a true case of the diffusion of nuclei. In this way the diffusion of about semi-saturation ($p = \cdot 5$) into benzol vapor initially about $\frac{1}{3}$ saturated would fully account for the apparent diffusion of nuclei into benzol vapor shown in the memoir cited.

6. Special experiments must therefore be made to decide whether when nuclei are added at the bottom of a homogeneous column of nearly saturated vapor, the observed diffusion is that of nuclei through the vapor, or of a greater concentration of vapor through homogeneous nucleation. For this purpose it is sufficient to add the nuclei in successive experiments at the top and at the bottom of the receiver, *A*, figure 1. The nuclei in such a case must diffuse alternately downward and upward, while the vapor diffuses upward only. These experiments, since made with care, showed that the addition of nuclei above or below the column of vapor is without effect on the observed diffusion. Hence it follows not only that the diffusion of the vapor and not of the nuclei has been observed, but that the nuclei must diffuse much more rapidly than the vapor. Indeed in the time in which the nuclei travel from top to bottom of the tall vessel nearly 1 meter high, the vapor has scarcely risen and the fog-bank seen on exhaustion lies close to the surface of the liquid.

An attempt to measure this rapid diffusion of the nucleus in benzol vapor by the present direct method failed, chiefly because all attempts to rigorously saturate the air in the receiver with the heavy vapor in a reasonable time were seriously hampered by convection. The results merely showed that the velocity of the nucleus in benzol vapor must be quite of the same order as in water vapor, but sharp data could not be obtained.

Brown University, Providence, R. I.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Solid Fluorine and its Combination with Liquid Hydrogen.*—Moissan and Dewar have shown, some time since, that fluorine liquefies at −187°, and that, at this low temperature, it does not act upon crystalline silicon, amorphous carbon, boron or mercury, but that it does combine violently with gaseous hydrogen and solid essence of turpentine. These investigators have recently subjected fluorine to the temperature of boiling hydrogen (−252·5°). As this cooling takes place the fluorine gas liquefies, then forms a yellow solid, and finally the solid becomes white. It was found that the melting-point of solid fluorine is about 2° higher than that of solid oxygen, or about −233°.

The interesting fact was observed that solid fluorine when brought in contact with liquid hydrogen at the temperature of the latter, −252·5°, produces a violent explosion. It appears, therefore, that chemical energy exists at this low temperature, which is only 20° above the absolute zero.

The solidification of fluorine leaves only one known gas, helium, which thus far has not been brought into the solid condition.—*Comptes Rendus*, cxxxvi, 641. H. L. W.

2. *Hydrides of Rubidium and Cæsium.*—Moissan has prepared the hydrides of the rare alkali-metals rubidium and cæsium, and finds that they correspond exactly to potassium hydride. They correspond to the formulas RbH and CsH, and form colorless crystals. Both compounds are decomposed by heating in a vacuum below 300° into hydrogen and metal. They take fire at ordinary temperatures in air, oxygen, chlorine, fluorine and bromine vapor. Heated in nitrogen gas they form mixtures of nitride and amide. They are decomposed rapidly by water at ordinary temperatures, but without incandescence, forming the metallic hydroxides and hydrogen. When acted upon by sulphur dioxide at low temperatures and diminished pressure they form the hydrosulphites $Rb_2S_2O_4$ and $Cs_2S_2O_4$. At a gentle heat carbon dioxide combines with them to form the formates. They react with ammonia gas and form the amides.—*Comptes Rendus*, cxxxvi, 587. H. L. W.

3. *Combustion in Gaseous Mixtures other than Air.*—Very conflicting statements have been made in regard to the amount of carbon dioxide which, when it is mixed with air, will extinguish a flame; for example, Eulenberg asserts that extinction occurs at 3 per cent of CO_2, Taylor at 10 per cent, Graham-Otto at 20 per cent, and Arnould at 25 per cent. Pelet and Jomini have recently made numerous experiments by using candle, alcohol, and illuminating-gas flames in mixtures of nitrogen and oxygen, carbon dioxide and oxygen, and also in mixtures of the three gases. After the flames had been extinguished the resulting mix-

tures were analyzed. It was found that the combustion of a flame can be maintained in the presence of varying quantities of carbon dioxide, up to as much as 75 per cent, so long as there remains a definite proportion of unburned oxygen in the mixture. For the same combustion the minimum proportion of unburned oxygen at which extinction occurs varies within narrow limits. For example, extinction took place in the following mixtures :

CO_2 Per cent	O_2 Per cent.	N_2 Per cent.
with the candle flame :		
3·6	16·0	80·2
10·8	17·0	72·2
27·8	19 5	52·7
50·0	21·2	27·9
78·	22·	
with the alcohol flame :		
6·7	14·3	79·0
20·3	13·0	66·7
with the gas flame :		
10·4	11·0	78·6
13·3	11·3	75·7
34·4	11·2	54·4

The interesting result is reached, therefore, that, contrary to the prevailing view, carbon dioxide has but little greater effect upon flames than the same amount of nitrogen, but it should be mentioned that in the investigation under consideration it was noticed that when combustion is taking place in the presence of a certain proportion of carbon dioxide, the hydrogen of the substance burns alone, forming water, while the flame smokes, and the carbon dioxide increases but little or not at all.—*Bull. Soc. Chem.*, xxix, No. 5. H. L. W.

4. *The Non-conductivity of Metallic Hydrides.* — MOISSAN has found that the hydrides of potassium, sodium, rubidium and cæsium in compact layers 5^{cm} long do not conduct the electric current, and that the fused hydrides of calcium and lithium behave in the same way. From these results the author concludes that hydrogen is not analogous to the metals, but corresponds to the non-metals, in these compounds, and that these hydrides cannot be considered as alloys of hydrogen with the metals.—*Comptes Rendus*, cxxxvi, 591. H. L. W.

5. *Heat Spontaneously Evolved by Radium Salts.*—It has been shown by CURIE and LABORDE that the salts of radium evolve heat continuously. When one junction of a thermo-electric couple was surrounded by radiferous barium chloride containing about one-sixth of pure radium chloride, and the other junction was similarly enclosed in pure barium chloride, a difference in temperature was indicated, which amounted to 1·5°. The amount of heat evolved in a given time was determined quantitatively by comparison with a wire of known resistance,

which was heated by a measured electric current until it produced the same effect as the radiferous material, and this value was determined also by Bunsen's calorimeter. It was thus found that 1 g. of radium gives off a quantity of heat amounting to about 100 small calories per hour. One gram-atom (225 g.) of radium would therefore give every hour, 22,500 cal., a quantity comparable to the heat evolved by the combustion of 1 gram of hydrogen in oxygen. The authors remark that the continuous disengagement of such a quantity of heat cannot be explained by an ordinary chemical change. If it is supposed that the production of heat is due to an internal transformation, this must be profound in its character, and must be due to a modification of the radium atom itself. However, if such a transformation exists, it takes place with extreme slowness, for the properties of radium do not undergo appreciable variations in the course of several years at least. Therefore, if the transformation hypothesis is true, the energy produced by the transformation of the atoms must be extraordinarily large. The hypothesis of a continual modification of the atom, however, is not the only one compatible with the disengagement of heat by radium, for the fact may be explained by supposing that radium utilizes some exterior energy of unknown nature.—*Comptes Rendus*, cxxxvi, 673. H. L. W.

6. *Analytical Chemistry;* by F. P. Treadwell. *Translated from the Second German Edition;* by William T. Hall. Vol. I, *Qualitative Analysis.* 8vo, pp. xi, 466. New York, 1903. John Wiley & Sons.—This is a rather elaborate work on qualitative analysis, possessing many excellent features. The descriptions of the operations are full and clear and the methods appear to be generally well chosen. However, the retention of the test for barium and strontium with calcium sulphate, which has generally been abandoned, and the omission of the very delicate potassium cobaltic nitrite test for potassium, have been noticed. The equations of the reactions are very fully treated, and the treatment of the acids is unusually extensive. A supplement deals with the rarer metals. Analytical tables, to which some teachers object, are used, but it is the author's experience that these give the best results. The translation appears to have been very well done, but a few errors, particularly in the equations, indicate some lack of care in proof-reading. H. L. W.

7. *Beiträge zur Chemischen Physiologie*, herausgegeben von F. Hofmeister. III. Band, 9–12. Braunschweig: Vieweg und Sohn. 1903.—The most notable contribution in the concluding numbers of this volume is the paper by Stoklasa and his coworkers, giving an account of the anærobic metabolism of the higher plants and its relation to alcoholic fermentation. It is demonstrated that the enzyme *zymase* is widely distributed in the vegetable kingdom, and that alcohol and carbon dioxide are metabolism products excreted by the vegetable cells even under conditions where micro-organisms are completely excluded. The

quantitative relation between the alcohol and carbon dioxide formed are the same as exists in the products of alcoholic fermentation with yeast. Several other papers deal with the chemical aspects of problems relating to immunity and the composition of proteid substances. L. B. M.

8. *Ionization produced by Corpuscles of Radium.*—Professor Townshend estimates that the value of this ionization must be at least 13. J. J. DURACK of Trinity College, Cambridge, believes that this number is seventy-six times too large. He finds that the mean free path of the Becquerel ray corpuscles in air at 1^{mm} is about 6^{mm}. This very large value accounts for the very small absorption of the deflectable Becquerel rays.—*Phil. Mag.*, May, 1903, pp. 550–561. J. T.

9. *Condensation of Radio-active effects.* — Professor RUTHERFORD and Mr. SODDY with the aid of liquid air show that the condensed emanations from thorium and radium possess a true vapor pressure and that the emanations have the usual properties of ordinary gaseous matter, as far as condensation and volatilization are concerned. These emanations are also occluded by solids under certain conditions and therefore seem to be gaseous in nature.—*Phil. Mag.*, May, 1903, pp. 561–576. J. T.

10. *Radio-active Change.* — Professor RUTHERFORD and Mr. SODDY, in a discussion upon the amount of energy stored in radio-active substances, hazard the suggestion that atomic energy in general is of a high order of magnitude and that the existence of this energy accounts for the stability of the chemical elements.

The maintenance of solar energy may be due to the available internal energy of the atoms.—*Phil. Mag.*, May, 1903, pp. 576–591. J. T.

11. *Removal of the Potential Difference by Heating in Oil.*—J. BROWN, F.R.S., in earlier papers has suggested that the difference of potential due to contact of metals might arise from the chemical action of films condensed on their surfaces. His recent work confirms this theory. On the first immersion of the metals in cold oil the films acted as if in air though with less deflection, due probably to minute conductivity of the oil, which reduced the potential difference. When heated to above the boiling point of water the films evaporated and left nothing to act electrolytically on the plates. After removal of the oil the moisture of the air found access to the plates and a difference of potential was again observed.—*Phil. Mag.*, May, 1903, pp. 591–594 J. T.

12. *On the Thickness of the Liquids formed by Condensation at the Surface of a Solid.*—Dr. G. J. PARKS refers to the previous knowledge in regard to condensation of liquids and gases on the surfaces of solids. The author was led to consider the quantity of moisture concerned in surface pressure, in the Pouillet effect, and in surface action in general. He concludes that in all cases where condensation takes place at a solid surface, and at a temperature not below the dew point, the thickness of the surface

film varies from 10×10^{-6} to 80×10^{-6}cm according to the substances used and the conditions of temperature and pressure, and for the water film on glass in saturated vapor at 15° the thickness is about 13×10^{-6}cm. The author quotes Professor B. J. Thomson's results for the mean radius of drops formed by condensation in electrified gas, namely 89×10^{-6}cm for negatively electrified oxygen and 68×10^{-6}cm for positively electrified oxygen.—*Phil. Mag.*, May, 1903, pp. 517–523. J. T.

13. *Speaking Flames.*—In a preliminary notice V. Gabritschewski and A. Batschinski describe a method of obtaining speech from flames. The poles of an induction coil were connected by wires to Bunsen burners or other flames. The primary circuit of the coil included a battery giving about four amperes and a microphone. The distance of the microphone was about 30m. The flames repeated various sounds and speech. It is essential that a sufficient difference of potential should arise at the flame electrodes.—*Ann. der Physik.*, p. 223. J. T.

14. *Vorlesung über Experimental Physik;* von August Kundt. Edited by Karl Scheel. Pp. xxiv, 852, with a half-tone of Kundt, 534 figures and a colored spectrum plate. (Braunschweig: Friedrich Vieweg u. Sohn.)—This volume, prefaced by a resumé of Kundt's life and labors, is the result of an earnest desire on the part of his former students to have, in accessible form, the lectures of this gifted investigator and teacher. It has been prepared from Kundt's notes of the lectures he delivered in Berlin ('88–'89) which bear traces of his own editing for publication. There are a total of 150 lectures covering the subjects of Mechanics, including Liquids and Gases, Sound, Heat, Electricity and Magnetism, and Optics—which are models of clearness and thoroughness of presentation.

The editor has preserved, so far as type permits, those characteristics of Kundt which gave him his hold upon the student. Every principle is developed in the simplest and most comprehensible manner from a consideration of the phenomena themselves, which he freely reproduced before his classes. This method is adopted in preference to a mathematical development, though the discussions always lead to the fundamental formulæ.

These lectures will undoubtedly prove of great interest and value to all teachers and students of the subject. D. A. K.

II. Geology and Natural History.

1. *New York State Museum:* Frederick J. H. Merrill, Director.—The following bulletins have recently been received:

No. 44 (vol. viii) Lime and Cement Industries of New York; by Heinrich Ries and Edwin C. Eckel. Pp. 639–968. 102 pls. The origin, composition, geologic occurrence and commercial uses of the New York limestones have been studied with great care and described in such a manner as to be of most use to quarry owners and limestone users. The chapters on the Portland Cement Industry, pp. 849–898, were written by Mr. Eckel.

No. 60 (Zoology 9). Fishes of New York; by TARLETON H. BEAN. 756 pp. The number of fishes included in this descriptive catalogue is 375. Of these species 217 are marine, 141 fresh water, and 17 anadromus. The small number of fresh-water species is due to lack of extensive investigations in the water bodies of the interior of the state.

2. *Geological Survey of New Jersey:* H. B. KÜMMEL, State Geologist. Annual Report, 1902. 149 pp. 5 pls.—Under the guidance of Dr. Kummel the New Jersey Survey is continuing its excellent work, chiefly along the lines of economic geology. The present volume contains reports on the Floods of Feb. 28 to March 5, 1902, by C. C. Vermeule; on Artesian Wells, by Lewis Woolman; on Forestry by F. R. Meier; on the Mining Industry, by Dr. Kümmel and by Mr. Weed. Mr. Knapp is at work on the coastal plain deposits.

3. *Geology and Petrography of the Crater Lake National Park;* by J. S. DILLER and H. B. PATTON. (Professional Paper No. 3, U. S. Geol. Surv., Washington, 1902, 4°, 167 pp., 19 pls.)—In the first portion of this work the geology is described by Mr. Diller. He gives a complete and detailed description of this remarkable body of water lying in a great crater in the top of Mt. Mazama, of the mountain itself and of its geological history and shows in various ways that the mountain was once a lofty peak which, collapsing, left a huge caldera in its place. The latter, filled with water, forms the present lake.

In the second part Professor Patton gives the results of a detailed and careful study of the igneous rocks of the mountain. He shows them to consist of the andesites, dacites and basalts of a number of different types, whose characters as shown by the study of thin sections and by chemical analyses are fully presented. The work is illustrated by half-tones of many fine photographs and in addition to its scientific interest will without doubt be of service as an authoritative handbook when this interesting region becomes quite accessible to the traveling public.

L. V. P.

4. *Preliminary Report on a part of the Granites and Gneisses of Georgia;* by THOS. L. WATSON, Ph.D. (Geol. Surv. Georgia, Bull. No. 9, 8°, 367 pp., 1902.)—This report is limited to a study of the granitic rocks in those counties in which there is a considerable technical production. In the first chapter the general characters of granite are treated; in the second, the properties of rocks in respect to technical purposes are discussed. The author then in the third chapter, after describing the general geology of the area, takes up the occurrences of granite with especial reference to the individual quarries, and in the succeeding one discusses the general chemical and petrographic characteristics of the Georgia granites. The volume concludes with a treatment of the subject of rock weathering with especial reference to the areas and rocks described. The volume shows a large amount of careful work, both in the field and in the laboratory, the results of 70

analyses of fresh and altered rocks being given, and is a useful addition to the general literature of scientific petrography as well as a fund of information for local technical purposes. It is well printed and illustrated by a large number of excellent half tone cuts and is a production creditable alike to the author and to the state survey. L. V. P.

5. *L'Age Des Formations Sédimentaires de Patagonie;* by FLORENTINO AMEGHINO. "Annals Soc. Cientif. Argentina," L and LIV, pp. 3–231, Buenos Aires, 1903.—It is not my intention to review this rather lengthy paper, the exact nature of the contents of which is scarcely intimated by the title. Nor does Dr. Ameghino's declaration on page 5 that "C'est une discussion exclusivement scientifique que ja vais tacher d'examiner en conscience et sans aucun préjugé," seem to have been kept constantly in mind throughout the succeeding 225 pages. The entire paper is devoted almost exclusively to a critical review of the literature of various authors upon the geology and paleontology of Patagonia, and the amount of space allotted to the three or four brief preliminary papers published by myself (this Journal, iv, 246–248; 327–354; ix, 85–108) is such that I ought certainly to feel flattered by the attention bestowed upon my work in Patagonia by Dr. Ameghino, the learned director of the National Museum at Buenos Aires. However, for the benefit of those who may not have access to Dr. Ameghino's paper, it is only proper for me to state in this connection that Dr. Ameghino's estimation of the value of my work in Patagonia is in inverse proportion to the space he has devoted to its criticism.

As to those matters relating purely to the geology and paleontology of Patagonia, wherein we differ, I shall have nothing to say at present. Having already stated my opinions and given in some detail my reasons therefor in my preliminary papers, I am content to await the publication of my final report on the geology of that region for a further elucidation of my views. In the meantime I must refer those interested in these questions to such of my publications as have already appeared, where they will find all important localities mentioned by me designated and described with such detail that they may be revisited and re-examined. I am so far from considering myself as infallible that I have always taken the precaution to preserve and publish detailed descriptions of all important localities in such manner that they may be easily revisited and identified by subsequent investigators. I shall always respect the opinion of any really competent observer, who revisits and re-examines any of the localities visited by me in Patagonia, regardless of whether his interpretation of the facts there displayed do or do not agree with those of myself. I do, however, believe that geology as a science is somewhat different from astronomy, and that it can only properly be studied at close range and not from a distance. Even so capable a man as Dr. Ameghino should, I believe, find

some difficulty in carefully determining the exact sequence of strata in Patagonia from the window of his study, situated in La Plata or Buenos Aires. Nor can I agree with Dr. Ameghino that he has satisfactorily answered my appeal to him for the publication of more exact localities by the reply given on page 8, that "Les recherches de C. Ameghino ont été faites à mes frais, pour mon instruction personelle et au profit de la science. Je n'avais pas l'obligation de faire des rapports détaillés. J'ai toujours tenu à donner des résumés des recherches géologiques de mon frère à fin que l'on put se rendre compte de la succession des faunes que je décrivais; mais je n'ai pas eu l'intention, et je n'en avais pas non plus l'obligation, de rédiger un guide avec les instructions nécessaires pour la récolto de fossiles."

I think those who are acquainted with me or my work will grant that I am fairly competent, as a collector of vertebrate and other fossils, and not one of the many who have applied to me in years past for localities will say that I did not at all times give them free and full information, nor in doing so have I ever considered that I was acting as a *guide*. I had no idea, when, on my first visit to Dr. Ameghino, he refrained from indicating to me on a map spread before him the location and distribution of his Pyrotherium beds, that he entertained the really *childish* ideas which seem to have inspired the lines quoted above, or I should never have urged his brother Charles when we were together at Santa Cruz in the winter of 1898, to join me in an expedition the following season to re-examine together the different Tertiary and Cretaceous horizons of Patagonia. Dr. Ameghino's remarks, just quoted, fully explain to me the reason why his brother, after fully agreeing with me as to the desirability of such a joint expedition and setting a time and place for our meeting the following spring to undertake the same, shortly after his return to La Plata wrote me that it would be impossible for him to keep the engagement, and that I must not wait for him to join me.

Since on page 18 of his paper Dr. Ameghino attacks my integrity and seems to infer that I have not acted in good faith in quoting the remarks of his brother relating to the manner in which he (Charles) had really found the mammalian and dinosaurian faunas of the *Pyrotherium* and *Guaranitic beds*, I hope I may be pardoned for a few brief remarks in defense of my position. After quoting me at some length Dr. Ameghino continues as follows: "Je repousse cette forme de discussion que, et j'en demande pardon à M. Hatcher, ne me parait pas trop correcte; il a supris la bonne foi de mon frère en lui raccontant que j'ai dit, ce qu'en vérité je n'ai jamais dit. C'est tout clair: il a demandé à Charles s'il avait trouvé des débris de mammifères associés à ceux de Dinosauriens, et il lui repondit, non. Si en place de cela, il lui aurait demandé s'il avait trouvé des débris de mammifères dans la même formation qui contient des os de Dinosauriens, certainement il lui aurait répondu, oui. Pourrait-il M. Hatcher m'indiquer, où, dans quel ouvrage, et à quelle page j'ai dit que C.

Ameghino a trouvé des os de mammifères associés à des os de Dinosauriens? Nulle part! La traduction de l'article dont parle M. Hatcher est parfaitement correcte, mais la traduction que lui en a donné à mon frère, qui ne lit pas l'anglais, est complètement inexacte. Dans l'article dont lui fait mention, tout ce qu'à ce sujet je dis, c'est: "That this formation is secondary, is clearly indicated by the Dinosaurs; on the other hand, as its upper beds pass insensibly into another formation, which contains numerous remains of mammals, it cannot be doubted that the sandstones with Dinosaurs belong to the upper Cretaceous."

"In Patagonia the beds with remains of Dinosaurs pass insensibly into other beds with numerous remains of mammals, particularly of ungulates, which circumstance proves that the red sandstones ought to be referred to the Upper Cretaceous."

It will be noticed that Dr. Ameghino does not accuse me of misquoting his brother's remarks to me. He also admits that the translation in the Geological Magazine of the article referred to is perfectly correct, but claims that my translation of it to his brother was completely incorrect. He denies ever having stated that C. Ameghino had found the bones of mammals associated with the bones of Dinosaurs and calls upon me to indicate in what work and on what page he has said this, closing with the exclamation, Nulle part! (No where). Now if Dr. Ameghino will but turn to page 10 of his "*Notes on the Geology and Paleontology of Argentina*" (the same article from which he quotes) as translated and published in the Geological Magazine for January 1897, which translation, he tells us, is perfectly correct, commencing with the first paragraph from the bottom he will find the following sentences: "I rely on the fact that these beds with remains of Pyrotherium everywhere accompany the red sandstones with remains of Dinosaurs, so that it has not hitherto been possible to separate them in an absolute manner. These sandstones in certain places exhibit nothing but bones of Dinosaurs; in others they show only remains of mammals and smaller reptiles of types not yet determined; *while at other points all these remains are shown mixed together* (italics mine), at least to all appearance, always accompanied by a great quantity of silicified wood." From this I think my readers will agree with me that the two short paragraphs quoted by Dr. Ameghino were not quite all that he said on this subject in that paper, and that I was fully justified in asking Charles Ameghino if he *had found* the remains of mammals associated with those of Dinosaurs, more especially since Dr. Ameghino has himself never seen any portion of Patagonia, and all that he has written on that country is based upon the observations of others and chiefly on those of his brother.

The same methods as those just shown are pursued throughout the entire paper, which abounds in quotations so adroitly made as to destroy the original meanings of the context, by substituting others never intended or implied by the various

authors criticised. Nor is this to be wondered at, for if Dr. Ameghino is so unfamiliar with his own published statements as to make the errors cited above, he can scarcely be expected to familiarize himself with the work of others. He affirms that I have never seen the *Pyrotherium beds*, and reproaches me, therefore, for having said anything about them, notwithstanding the fact that his brother Charles distinctly stated to me that certain deposits on the upper Rio Shehuen, with which I am acquainted, pertain to these beds. Not having myself found any characteristic fossils in these deposits, I relied entirely upon the determinations of Charles Ameghino as to their identity, and I still have no good reason for doubting the correctness of his determinations, but every reason for doubting the age ascribed to them by Dr. Florentino Ameghino.

Again, in speaking of the lakes of Patagonia, Dr. Ameghino reproaches me, and not without some reason, for venturing to say anything concerning Lakes Colhue and Musters, neither of which I have ever seen. This fact, one would infer, is in Dr. Ameghino's opinion sufficient reason why I should have remained silent. Indeed I am half inclined to agree with Dr. Ameghino in this matter, and I only wonder why it has never occurred to him to apply the same rule of stricture to his own publications, for I fully realize the confusion that might thereby have been avoided concerning the geology of Patagonia, a country he has never visited. J. B. HATCHER.

6. *Catalogue of Meteorites in the Vienna Museum.* — Prof. F. BERWERTH has published (Annalen des k. k. naturhistorischen Hofmuseums, Wien, Bd. xviii) a catalogue, with two appendices, of the meteorites in the Vienna Museum at the close of October, 1902. The catalogue shows that this collection still retains its rank as the largest in the world, the total number of falls represented being 560, and the total weight of cosmic matter exhibited 3,313 kilograms, of which 2,553 kilograms are meteoric irons, 122 kilograms ironstones, and 637 kilograms stones.

In the first part of the catalogue the falls represented in the Vienna collection are grouped chronologically under the separate divisions of irons, iron-stones and stones. Relegation of Santa Catarina to the group of pseudo-meteorites is a noteworthy change here and one with which most students of meteorites will agree. On the other hand, the grouping of the meteorites of Prairie Dog Creek, Jerome, Kansada and Ness County under the one head of Prairie Dog Creek, is not likely to meet with unqualified approval.

The first appendix gives an alphabetically arranged list of all known meteorites, with their classification, date of fall or find, and latitude and longitude of the locality from which each was obtained. It is to be regretted that the latter feature seems somewhat lacking in accuracy, errors of several degrees being observable in more than one case. In choosing names for the falls, Prof. Berwerth has followed in general the plan of giving

the meteorite the name under which it was originally described, rather than that of the nearest locality. Priority is thus deemed of more importance than geographical position, and a somewhat elaborate defense of this course is given in the preface. Its advisability may, however, be questioned, especially as it causes many American falls to be named from counties rather than from towns.

In the second appendix a list of known meteorites by countries is given, a work which, like the catalogue as a whole, affords information of great usefulness to students of meteorites.

O. C. F.

7. *The Eruption of Colima.* (Communicated by F. L. SPERRY.)—"The volcano of Colima in Mexico has been recently in eruption and the following data in regard to it may be of value to students of volcanic phenomena. They were obtained during a recent visit to the city of Zapotlan, Jalisco, Mexico. The volcano is about 100 miles from the west coast and about 25 miles from Zapotlan. Its height is about 12,500 feet and it is a well known landmark. The history of the volcano according to residents of the city is as follows: There was a great eruption accompanied by earthquakes on Oct. 22d, 1742. The next was on March 25th, 1806, with heavy earthquakes which destroyed a church, killing 2,000 of the inhabitants who had sought refuge there. In 1856 there were several eruptions and earthquakes which destroyed houses in Zapotlan, Tuxpan and other places within a radius of 30 miles. The volcano was smoking last July, sending up pillars of steam high in the air, at irregular intervals. At 12.30 P. M. Feb. 18th, 1903, the inhabitants of Zapotlan were startled by a mighty roaring accompanied by slight shocks and the fall of ashes. This was repeated Feb. 20th at 12.30 P. M. At 4 P. M. Feb. 24th there came a fearful eruption with showers of ashes, which shrouded the city in darkness for three hours. These eruptions continued every three or four days until March 9th, when the greatest spectacle of all took place and the material of the eruption was thrown into the city. The fine ash and gases formed a very solid column reaching high in the atmosphere, which remained intact for more than an hour.

On March 10th this spectacle was repeated, then the volcano remained dormant until March 23d at 1 P. M., when there was an eruption which continued for half an hour accompanied by wonderful spectacular effects. Since then the volcano has remained quiet."

(A specimen of the volcanic ash of the eruption of March 9th, collected in Zapotlan and sent to the editors by Mr. Sperry, shows a light gray andesitic material averaging about 1^{mm} in diameter. Examination with the microscope reveals a mass of minute lapilli-like particles angular to subangular, in part vesicular, in part consisting of solid particles of feldspars. The vesicular portion is sometimes of glass but mostly of lithoidal lava. It is too fine to be called lapilli and is rather coarse for "ash." The

fact that material of this nature could be driven the distance from the volcano to the city testifies to the violence of the eruption.) L. V. P.

8. *Report on the Eruptions of the Soufrière in St. Vincent in 1902, and on a Visit to Montagne Pelée, in Martinique*, Part I; by TEMPEST ANDERSON and JOHN S. FLEET. Phil. Trans. Royal Soc., Series A, vol. cc, pp. 353–553, pls. 21–39.—The first part of the report by Doctors Anderson and Fleet contains the following chapters: Physical Features and Geology; the Eruption of May 7th; the Eruption of May 18th; Rains; Geological Effects of the Eruptions; Previous Eruptions of the Soufrière; the Soufrière and Montagne Pelée; Comparative study of the Peléan type of Eruption; General Sequence of volcanic phenomena in the Antilles and Central America in the early part of 1902. The account is full of interest and the illustrations are of very high order.

9. *Megablattina Sellards (non Brongniart): A Correction.* (Communicated by E. H. SELLARDS.)—The name *Megablattina* proposed for a new genus of cockroaches from the Coal Measures in the April number of the current volume of this Journal, p. 312, I now find to be preoccupied, having been applied by Brongniart in 1885 to an insect from the Saarbrucken Basin, Germany, previously described by Goldenberg under the hemipterous genus *Fulgorina*. I now suggest for the American genus the name *Archoblattina*.

Yale Museum, May 5, 1903.

10. *The Fauna and Geography of the Maldive and Laccadive Archipelagoes.* Edited by J. STANLEY GARDINER. Vol. I, Part IV, pp. 347–471, 43 figs., 4 pls.—This completes the first volume (this Journal, xiii, 321; xiv, 74; xv, 240). Appendices B and C conclude Mr. Gardiner's valuable reports on the Coral Reefs. Others on the Cephochorda and Lithothamnia are of especial interest. C. F. Cooper gives two species of Amphioxus,—the West Indian *Asymmetrum lucayanum* (Andrews), which is very common, and the new and rare form, *Heteropleuron Maldivense*; also some undetermined larvæ. R. C. Punnett gives interesting results of his study of the "Meristic variation" of the group, showing that increase in number of gonads is associated with decrease in number of segments, and suggests that *genetic selection* may prove a factor of importance in modifying these features. M. Foslie reports nine forms of the little known calcareous algæ (Lithothamnia), none having ever before been recorded from the area between the Red Sea and the East Indies. H. Gadow and J. S. Gardiner give a list of twenty-six birds. F. E. Beddard records three species of earthworms, one of which is new. L. A. Borradaile continues his study of the marine crustaceans, reporting on the Brachyura, proposing one new genus *Selwynia*, type *Selwynia lævis* new, and the Cirrepedia. W. F. Lanchester reports on the Stomatopoda. K. J. B.

11. *Catalogue of the Collection of Palæarctic Butterflies formed by the late John Henry Leech;* by RICHARD SOUTH. British Museum, 229 pp., 2 colored pls.—The collection made by Mr. Leech comprises 18,000 specimens representing 1100 species.

12. *The Determination of the Parallax of the Ten First Magnitude Stars in the Northern Hemisphere;* by WM. L. ELKIN. Trans. Astronomical Observatory of Yale University, vol. i, pt. 6, pp. 259–330.—This publication gives the details of the results of one part of a plan entered into some years ago by the author and Sir David Gill, Her Majesty's astronomer at the Cape of Good Hope, by which the latter was to make a similar determination for the brightest southern stars.

The work was begun in 1885 and the original plan of securing some fifty heliometer measurements of each star was completed in 1891. A discussion of the results showed that the hoped-for degree of accuracy had not in every case been attained, and accordingly further series were made extending through the year 1894.

The author in his characteristic, painstaking manner, has discussed every possible source of error and has deduced the best results possible from his accumulated material. His final results are as follows:—

α Tauri ..... π	$= + 0''\cdot109 \pm 0''\cdot014$
α Aurigæ	$= + 0''\cdot079 \pm 0''\cdot021$
α Orionis	$= + 0''\cdot024 \pm 0''\cdot024$
α Canis minoris	$= + 0''\cdot334 \pm 0''\cdot015$
β Geminorum	$= + 0''\cdot056 \pm 0''\cdot023$
α Leonis	$= + 0''\cdot024 \pm 0''\cdot020$
α Bootis	$= + 0''\cdot026 \pm 0''\cdot017$
α Lyusis	$= + 0''\cdot082 \pm 0''\cdot016$
α Aquilæ	$= + 0''\cdot232 \pm 0''\cdot019$
α Cygni	$= - 0''\cdot012 \pm 0''\cdot023$

In the probable errors here given is included an estimated systematic error, which has increased the magnitude of the probable errors about fifty per cent.

The small result found for Arcturus (α Bootis) was somewhat surprising in view of the well known large proper motion possessed by this star, which would naturally lead us to think it one of the nearest. It was at first thought the anomalous result might possibly be due to the star's color, but investigations upon other red stars made at the Yale Observatory and by several other heliometer observers, as well as a special analysis by the author of his own measures of this star, indicate that this can hardly be the explanation. It is understood that a further series of observations of this star, to be carried out on somewhat different lines, is now in progress at the Yale Observatory. C.

III. Miscellaneous Scientific Intelligence.

1. *National Academy of Sciences.* — The annual meeting of the National Academy of Sciences was held in Washington, April 21, 22 and 23. President Ira Remsen was elected vice president and Professor Simon Newcomb foreign secretary. The following gentlemen were elected members: T. C. Chamberlin of Chicago, William James of Cambridge, E. L. Mark of Cambridge, Arthur G. Webster of Worcester, Horace L. Wells of New Haven.

The following papers were presented:

Henry F. Osborn: An estimate of the weight of the skeleton in the Sauropoda, or in the Sauropodous Dinosaurs. New characters of the skulls of Carnivorous and Herbivorous Dinosaurs. Models illustrating the evolution of the Amblypoda, also of the Dinosaur *Diplodocus*, together with a theory as to the habits of the Sauropoda.

George F. Barker: Radio-activity of thorium minerals.

J. M. Crafts: The law of catalysis in concentrated solutions. The standardization of thermometric measurements.

George E. Hale: The Rumford spectroheliograph of the Yerkes Observatory.

Lewis Boss: The determination of standard right-ascensions free from the personal equation for star-magnitude.

R. A. Harris: On the semi-diurnal tide of the northern part of the Indian Ocean.

Arthur L. Day: The melting point of a simple glass.

Carl Barus: The diffusion of vapor into nucleated air.

H. P. Bowditch: Biographical Memoir of Theodore Lyman.

Alexander Agassiz: The nomenclature of the topography of the bottom of the oceans.

S. Weir Mitchell: On the discovery of an antidote for rattlesnake poison.

Alexander Graham Bell: On the tetrahedral principle in kite structure.

Theodore Gill: Biographical Memoir of J. E. Holbrook.

George F. Barker: Biographical Memoir of Matthew Carey Lea.

S. F. Emmons: Biographical Memoir of Clarence King.

Jeffries Wyman: Biographical Memoir of A. A. Gould.

Charles S. Hastings: Biographical Memoir of James E. Keeler.

2. *International Catalogue of Scientific Literature.* — First Annual Issue: A. Mathematics, May, 1902. Pp. xiv, 201. B. Mechanics, July, 1902. Pp. xiv, 128. C. Physics, Part I, February, 1902. Pp. xiv, 239. E. Astronomy, May, 1902. Pp. xiv, 303. F. Meteorology, including Terrestrial Magnetism, May, 1902. Pp. xiv, 184. Q. Physiology, including Experimental Psychology, Pharmacology and Experimental Pathology, Part I, February, 1902, xiv, 404. R. Bacteriology, May, 1902, xiv, 314.—Seven volumes of the International Catalogue of Scientific Literature have recently appeared. They are in every way worthy of the Royal Society, under whose auspices they are published, and are of very great value to the scientific worker. The details of the plan of publication, cost, etc., are given in the preface to each volume. (See, also, this Journal, xiv, 317, October, 1902.)

3. *Carnegie Institution of Washington.* Year Book No. 1, 1902, pp. i–xlvi, 1–305.—The founding of the Carnegie Institution for the promotion of study and research by a gift of ten millions of dollars, given by Andrew Carnegie in January, 1902, is known to all students of science. This first year book contains copies of the official documents by which the Institution was established and minutes of the meetings, held by the trustees during the first year of incorporation. The proceedings of the Executive Committee are also given in abstract, of which the more important items are the announcement of the composition of the several Advisory Committees, and definite statements of the purposes, principles, organization and policy of the Institution.

The plans and methods thus far agreed upon may be indicated by quotation of a few extracts from the Summary at the close of this portion of the book, viz: "The methods of administration of the Carnegie Institution thus far developed are general rather than specific; . . . Attention has been concentrated upon a selection of those objects which, at this time and in our country, seem to require immediate assistance; . . . Efforts have been and will be made to secure coöperation with other agencies established for the advancement of knowledge, while care will be exercised to refrain from interference or rivalry with them; . . . Specific grants have been and will be made, for definite purposes, to individual investigators, young and old, of marked ability, and for assistance, books, instruments, apparatus, and materials."

Provision is made to enable highly qualified persons to profit by the advantages for research afforded by the various scientific bureaus of the United States Government at Washington,—for the appointment of research assistants annually to make special investigations—for publishing the results of research. Preliminary inquiries will be made regarding proposed large undertakings to determine their prospective value and cost. Special advisers have been and will be invited from time to time to report as experts upon questions submitted to them. At the meeting of the Board of Trustees, November 25, 1902, $50,000 was appropriated for administration expenses, $200,000 for grants for research during the fiscal year 1902–3, $40,000 for the publication fund, and $100,000 was set apart as a reserve fund.

Following the official report are two appendices, consisting of A, Report of Advisory Committees, and B, Proposed Explorations and Investigations on a large scale. Eighteen advisory committees were appointed, and the fields of research thus emphasized indicate the scope of science which is covered at the present time by the operation of the Carnegie Institution. The subjects upon which these advisory committees reported are as follows, viz: economics, botany, physics, geology, geophysics, geography, meteorology, chemistry, astronomy, paleontology, zoology, physiology, anthropology, bibliography, engineering, psychology, history and mathematics. In addition to these the special proposals in Appendix B are for biological surveys of the

Palearctic region, of South and Central America, of the Antarctic, and of the ichthology of the Pacific Ocean; for the establishment of biological experiment stations for the study of problems of evolution; and for making investigations of subterranean temperatures and gradients.

The specific recommendations made by the several advisory committees are significant as indicating the particular trend of scientific investigation at the present time. Among these some of the more important are the following, viz: In botany, relation of vegetation to environment, research in the West Indies; in geophysics, distribution of terrestrial density, upheaval and subsidence in organic dynamics, the operative causes of vulcanism, diathermacy of the atmosphere, tidal deformation, deformation of rocks under stress, mass distribution and variation in density of the earth, gases held in magmas, physical chemistry of natural solutions and precipitates, relation of pressure to melting point; in geography and meteorology, investigation of the upper atmosphere; in astronomy, solar observatories and the study of quantity and nature of solar radiation, establishment of observatories in the southern hemisphere, celestial photometry and photography, astrophysical research as to the physical and chemical constitution of heavenly bodies, and the tracing out and explanation of the successive stages in their evolution from nebulæ, determination of the question, does the force of gravitation vary exactly as the inverse square; in paleontology, the establishment of standard bench-marks for the geological time scale; in anthropology, establishment of laboratories for the study of development of physical types of men, the archæology of native peoples of America, the systematic study of the vanishing tribes of American aborigines; in psychology, investigation in comparative or zoological psychology, and in anthropological and social psychology; in history, an institute for historical research in Washington, and the conducting of research of American history among European archives; in biology, experimental forms and laboratories for studying problems of evolution, especially as to the effects and conditions of environment upon the development and evolution of organisms, and broad explorations in various parts of the earth to determine the laws of distribution and migration and origin of the present faunas and floras of the earth. The administration of the Carnegie Institution is placed in the hands of a Board of Trustees composed of twenty-seven members, who chose John S. Billings, Daniel C. Gilman, Abram S. Hewitt, S. Weir Mitchell, Elihu Root, Charles D. Walcott and Carroll D. Wright to be its first Executive Committee, and Daniel C. Gilman, President, and Charles D. Walcott, Secretary.

H. S. W.

Obituary.

Josiah Willard Gibbs, Professor of Mathematical Physics in Yale University, died April 28, aged 64. (A biographical notice will appear later.)

INDEX TO VOLUME XV.*

A

Academy, National, meeting at Washington, 491.
Agassiz, A., coral reefs of the tropical Pacific, 333.
Andrews, E. F., Botany all the Year Round, 334.
Argentino-Chilian Boundary, 331.

B

Baldwin, J. M., Development and Evolution, 88.
Barus, C., ionization of water nuclei, 105; ionization of water and phosphorus nuclei, 217; apertures of coronas, 335; diffusion of vapor into nucleated air, 472.
Bauer, L. A., Magnetic Declination Tables and Isogonic Charts, 1902, 157.
Bermuda Islands, Verrill, 332; zoology of, Verrill, 333.
Birds of North and Middle America, Ridgway, 418.

BOTANY.

Botanical Survey of San Jacinto Mts., Hall, 87.
Cyperaceæ, studies, xviii, Holm, 145.
Flora of the South Fork of Kings River, Eastwood, 86.
Fungus Diseases of Stone-Fruit Trees in Australia, McAlpine, 239.
Phycologia Boreali-Americana. Collins, Setchell and Holden, 239.
Plankton des Norwegischen Nordmeeres, Gran, 240.
Plants, electromotive force in, Plowman, 94.
Botany all the Year Round, Andrews, 334.
Boys radiomicrometer, Hutchins, 249.
Browning, P. E., ceric chromate, 177.
Brückner, E., die Alpen in Eiszeitalter, 238.
Burbank, J. E., surface tension and viscosity, 140.

C

California, botanical survey of San Jacinto Mts., Hall, 87.
— flora of Kings River, Eastwood, 86.
— Klamath Mountain section, Diller, 237, 342.
Campbell, H. D., meteoric iron from Virginia, 469.
Carnegie Institution, Washington, 492.
Chant, C. A., variation of potential along a wire transmitting electric waves, 54.
Chemie, physikalische, der Zelle, etc., Höber, 80.
Chemische Physiologie, Hofmeister, 235, 479.
Chemistry, Analytical, Treadwell, 479.
— Physical, Cohen, 326.

CHEMISTRY.

Anhydrous hydrosulphites, Moissan, 234.
Arsenic, antimony and tin, qualitative separation, Walker, 324.
— pentachloride, Baskerville and Bennett, 324.
Boric acid, solubility, Hertz, 235.
Cæsium, atomic weight, Richards and Archibald, 415.
Carbon, combustion of three varieties of, Moissan, 155.
— in steel, determination of, Sargent, 326.
Ceric chromate, Browning and Flora, 177.

* This Index contains the general heads, BOTANY, CHEMISTRY (incl. chem. physics), GEOLOGY, MINERALS, OBITUARY, ROCKS, ZOOLOGY, and under each the titles of Articles referring thereto are mentioned.

Cupric chlorides, ammoniacal, Bouzat, 155.
Electrolytic peroxide of lead, nickel, etc., Hollard, 325.
Fluorine, solid, Moissan and Dewar, 477.
Gaseous mixtures, combustion in, Pelet and Jomini, 477.
Hydrides, alkali-metal, Moissan, 154.
— non-conductivity of metallic, Moissan, 478.
— of rubidium and cæsium, Moissan, 477.
Hydrochloric acid, action on aluminum, etc., Recoura, 154.
Hydrogen peroxide, crystallized, Städel, 233.
Iron, preparation of pure, Skrabal, 235.
Oxidizing agents, new reaction for certain, de Koninck, 234.
Ozone, formation by electrical discharges, Warbury, 81.
Phosphorus, emanations of, Schmidt, 416.
— free, in meteorite, Farrington, 71.
Potash salts, Groth, 80.
Radium, properties, Giesel, 78.
— salts, heat evolved by, Curie and Laborde, 478.
Silver iodide, double salt of, Strömholm, 325.
Tellurium, radio-active, Marckwald, 154.
Thorium, radio-activity, Rutherford and Soddy, 79.
Uranium, radio-activity, Soddy, 78.
Vanadic acid, estimation of, Gooch and Gilbert, 389.
Chwolson, O. D., Lehrbuch der Physik, 82.
Clarke, F. W., pseudo-serpentine from Washington, 397.
Cockroaches, Paleozoic, Sellards, 307, 489.
Cohen, E., meteoric iron from the Soudan, 254.
Cohen, Ernst, Physical Chemistry, 326.
Colima, Mexico, eruption of, Sperry, 488.
Collins, F. S., Phycologia Boreali-Americana, 239.
Colorado, Rico Mts., ore deposits of, Ransome, 158.
Coronas, apertures of, Barus, 335.
Crater Lake, National Park, geology, Diller and Patton, 483.
Cumings, E. R., morphogenesis of Platystrophia, 1, 121.

D

Daly, R. A., mechanics of igneous intrusion, 269.
Davis, B., conductivity and energy absorption in the electrodeless discharge, 202.
Davis, H. N., Newton's colors shown by transmitted light, 224.
DeLaunay, L., Richesses Minérales de l'Afrique, 417.
Development and Evolution, Baldwin, 88.
Diamonds, see **Minerals**.
Dickson, C. W., platinum in the nickel-copper ores, Sudbury, Ontario, 137.
Diller, J. S., Klamath Mountain section, California, 342.
Dyar, H. G., North American Lepidoptera, 418.

E

Earthquake investigation committee, Japan, 239.
Eastwood, A., Flora of South Fork of Kings River, 86.
Eaton, G. F., Triassic fishes, collection at Yale, 259.
Electric conductivity of electrodeless discharge, Davis, 202.
— currents of low potentials, fatal effect, 236.
— properties of thin metal films, Patterson, 80.
— waves, variation of potential along wire transmitting, Chant, 54.
Electricity carried by a gaseous ion, Thomson, 327.
Electro-chemical analysis, Smith, 80.
Electrolytic determination of metals, use of rotating cathode, Gooch and Medway, 320.
Electro-Metallurgie, Bröchers, 80.
Electromotive force in plants, Plowman, 94.
Elkin, W. C., parallax of ten first magnitude stars in Northern hemisphere, 490.
Ether, can its motion cause double refraction? Rayleigh, 81.
Evans, N. N., native arsenic from Montreal, 92.
Ewell, A. W., rotatory polarization mechanically produced, 363.

F

Farrington, O. C., free phosphorus in meteorite, 71.

Fenneman, N. M., Lakes of southeastern Wisconsin, 238.
Flora, C. P., ceric chromate, 177.
Ford, W. E., rickardite, 69; chemical composition of axinite, 195.

G

Gardiner, J. S., Maldive and Laccadive Archipelagoes, 240. 489.
Gases, conductivity produced in, by ultra-violet light, Townsend, 415.

GEOLOGICAL REPORTS.

Louisiana, Harris, 330.
New Jersey, 1902, 483.
United States, 22d annual report, 157, 328. Bulletins 196-199, 237; Nos. 200-204, 328.
Vermont, 1901-1902, Perkins, 329.

GEOLOGY.

Alpen, die, in Eiszeitalter, Penck and Brückner, 238.
Brachiosaurus altithorax, Riggs, 299.
Cycadophyten, mesozoische, Nathorst, 85.
Dinosaur, largest known, Riggs, 299.
Elkhorn Mining District, Montana, Weed and Barrell, 158.
Ellensburg Folio, Washington, Smith, 328.
Eocene Mammalia in Marsh Collection, Wortman, 163, 399, 419.
Evolution of the horse, Gidley, Matthew, 161.
Flora, fossil, of the Arctic, Nathorst, 85.
Formations sedimentaires de Patagonia, Ameghino, 484.
Furmarole gases from Mount Pelée, Moissan, 233.
Geological structure of Bolson Plains, New Mexico, Keyes, 207.
— — of southwestern New England, Hobbs, 437.
Geology of Crater Lake, National Park, Diller and Patton, 483.
— and water resources of the Snake River plains, Idaho, Russell, 238.
Gold belt of Blue Mts., Oregon, Lindgren, 158.
John Day Basin, Oregon, fossil flora of, Knowlton, 328.
Klamath Mountain section, California, Diller, 237, 342.
Lead and zinc deposits, Ozark Region, Van Hise, Bain, Adams, 158.
Marine turtles, Wieland, 211.
Mylagaulodon, new rodent from Oregon, Sinclair, 143.
Niagara domes of northern Indiana, Kindle, 459.
Ore deposits of Monte Cristo, Washington, Spurr, 159; of Rico Mts., Colorado, Ransome, 158.
Paleozoic brachiopod, study of, Cumings, 1, 121.
— cockroaches, Sellards, 307. 489.
Platystrophia, morphogenesis of, Cumings, 1, 121.
Primates, Eocene, in Marsh Collection, Wortman, 163, 399, 419.
Triassic fishes, collection at Yale, Eaton, 259.
Urals, geology and petrography, 83.
Georgia, granites and gneiss of, Watson, 483.
Gilbert, R. D., use of zinc reductor in estimation of vanadic acid, 389.
Glacial formations of the Erie and Ohio Basins, Leverett, 160.
— geology of New Jersey, Salisbury, 418.
— period, cause of, True, 161.
Glaciers, periodic variations of, 160.
Goldschmidt, V., new forms of sperrylite, 83, 450.
Gooch, F. A., use of the rotating cathode in the electrolytic determination of metals, 320; use of zinc reductor in estimation of vanadic acid, 389.
Good seeing, Langley, 89.
Gratings, concave, Rollins, 49.
Gravitation energy, absorption by radio-active substances, Kaufmann, 415.
Groth, L. A., Potash Salts, 80.

H

Harrington, B. J., composition of some Canadian amphiboles, 392.
Hatcher, J. B., review of Ameghino's Patagonia, 484.
Hobbs, W. H., geological structure of southwestern New England, 437.
Höber, R., Physikalische Chemie der Zelle, etc., 80.
Hofmeister, F., Beiträge zur chemischen Physiologie, 235, 479.
Holden, I., Phycologia Boreali-Americana, 239.
Holm, T., studies in the Cyperaceæ, xviii, 145.
Horse, evolution of, Matthew, 161; tooth characters and revision of, Gidley, 161.

Howe, H. M., Metallurgical Laboratory Notes, 79.
Howe, J. L., meteoric iron from Virginia, 469.
Hutchins, C. C., Boys radiomicrometer, 249.

I

Idaho,—Snake River Plains, geology and water resources, Russell, 238.
Index Animalium, Sherborn, 334.
Indiana, Niagara domes of northern, Kindle, 459.
Ion, charge on, produced in air by Röntgen rays, Wilson, 416.
— gaseous, carrying electricity, Thomson, 327.
Ionization produced by corpuscles of radium, Darack, 480.
— of water nuclei, Barus, 105, 217.
Iron spectrum, photography of ultrared, Lehmann, 156.

K

Keyes, C. R., geological structure of Bolson Plains, New Mexico, 207.
Kindle, E. M., Niagara domes of northern Indiana, 459.
Kundt, A., Experimental Physics, 481.

L

Langley, S. P., "Good seeing," 89; report of Smithsonian Institution, 242.
Leverett, F., Glacial formations of Erie and Ohio Basins, 160.
Light, interference of, Lummer and Gehrcke, 326.
Liquids, thickness of, by condensation, Parks, 480.
Loomis, H. B., change of temperature on permanent magnets, 179.
Louisiana, report on the geology, Harris, 330.

M

Magnetic Declination Tables and Isogonic Charts, 1902, Bauer, 157.
Magnetometer, torsion, Kohlrausch and Holborn, 236.
Magnets, change of temperature on permanent, Loomis, 179.
Maldive and Laccadive Archipelagoes, Gardiner, 240, 489.
Mann, C. R., Optics, 157.
Marsh Collection, studies of Eocene Mammalia in, Wortman, 163, 399, 419.
McAlpine, D., Fungus Diseases of Stone-Fruit Trees in Australia, 239.
Mechanics, Molecular Physics and Heat, Millikan, 327.
Medway, H. E. use of rotating cathode in electrolytic determination of metals, 320.
Metal films, electrical properties, Patterson, 80.
Metallic vapor, reversed lines of, 416.
Metallurgical Laboratory Notes, Howe, 79.
Meteorite, free phosphorus in, Farrington, 71.
— iron, Soudan, Cohen, 254; Virginia, Campbell and Howe, 469.
— stone, Andover, Mass., Ward, 395; Bath Furnace, Kentucky, Ward, 316.
Meteorites in the Vienna Museum, catalogue, Berwerth, 487.
Miers, H. A., Mineralogy, 84.
Millikan, R. A., Mechanics, Molecular Physics and Heat, 327.
Minérales de l'Afrique, les Richesses, De Launay, 417.
Mineralogy, Miers, 84.
Minerals, Tables of, Penfield, 330.

MINERALS.

Amphiboles, composition of some Canadian, 392. Anapite, 82. Arsenic, native, Montreal, 92. Axinite, composition, 195.
Brunsvigite, 417.
Chalmersite, Brazil, 83.
Diamonds, artificial, from fused silicates, 153; fluorescence and phosphorescence, 153.
Hudsonite, 227.
Koenenite, 83.
Liveingite, 417.
Platinum in the nickel-copper, Sudbury ores, Ontario, 137. Pyknochlorite. 417.
Quartz, expansion of melted, Holborn and Henning, 236.
Rickardite, new, Vulcan, Colorado, 69.
Serendibite, Ceylon, 417. Serpentine, pseudo-, Washington, 397. Sperrylite, new forms, Ontario, 83, 450.

Montana, Elkhorn Mining District, Weed and Barrell, 158.
Mt. Pelée, fumarole gases from, Moissan, 233; eruption, Anderson and Fleet, 489.

N

Nathorst, A. G., Fossile Flora der Polarländer, 85; Mesozoische Cycadophyten, 85.

New Jersey geological survey, 1902, 483; Glacial Geology, Salisbury, 418.
New Mexico, geological structure of Bolson Plains, Keyes, 207.
Newton's colors shown by transmitted light, Davis, 224.
New York State Museum, 481.
Nicol, W., new forms of sperrylite, 83, 450.
Nuclei, ionization of water, Barus, 105, 217.

O

OBITUARY—
d'Archiardi, A., 242.
Belknap, G. E., 418; Bell, C. J., 242.
Chester, A. H., 418.
Fuertes, E. A., 242.
Gibbs, J. W., 493.
Harkness, W., 334; Hill, H. B., 418.
Rood, O. N., 73.
Stokes, G. G., 242.
Oil, potential difference by heating in, Brown, 480.
Optics, Advanced, Mann, 157.
Oregon, Gold Belt of Blue Mts., Lindgren, 158.
— John Day Basin, fossil flora of, Knowlton, 328.
— Mylagaulodon from, Sinclair, 143.
Ozark Region, lead and zinc in, Van Hise, Bain, Adams, 158.

P

Palæarctic, butterflies of, J. H. Leech, catalogue, South, 490.
Patagonia, age of sedimentary formations, Ameghino, 484.
— Princeton University expedition to, Hatcher, 331.
Penck, A., die Alpen in Eiszeitalter, 238.
Penfield, S. L., Tables of Minerals, 330.
Pennsylvania, Masontown-Uniontown Folio, Campbell, 328.
Photo-Micrography, Walmsley, 327.
Physical papers of H. A. Rowland, 237.
— Society, American, 327.
Physik, Experimental, Kundt, 481.
— Lehrbuch der, Chwolson, 82.
Platinum in Sudbury ores, 137.
Plowman, A. B., electromotive force in plants, 94.
Polarization, rotary, mechanically produced, Ewell, 363.
Potential distribution in dark cathode space, Wehnelt, 327.
— variation along a wire transmitting electric waves, Chant, 54.
Princeton University Expedition to Patagonia, Hatcher, 331.
Probabilities, philosophical essay on, Simon, 162.

R

Radiation phenomenon, Graetz, 155.
Radio-active change and effects, Rutherford and Soddy, 480.
— substances and gravitation energy, Kaufmann, 415.
Radio-activity, excited, and its transmission, Rutherford, 156.
— of tellurium, Marckwald, 154; thorium, Rutherford and Soddy, 79; uranium, Soddy, 78.
Radium, Bunsen flame of, Runge and Precht, 326; in the periodic system, Runge and Precht, 416; properties of, Giesel, 78.
Ridgway, R., Birds of North and Middle America, 418.
Riggs, E. S., Brachiosaurus altithorax, 299.

ROCKS.
Basalts of the Rhine, Zirkel, 331.
Granites and gneiss of Georgia, Watson, 483.
Koswite, 84.
Mechanics of igneous intrusions, Daly, 269.
Petrography of the Urals, DuParc and Pearce, 83.
Rollins, W., ruling concave gratings, 49.
Rood, Ogden Nicholas, obituary notice, Wright, 73.
Rowland, H. A., physical papers of, 237.
Russell, geology and water resources of Snake River Plains, Idaho, 238.

S

Salisbury, R. D., Glacial Geology of New Jersey, 418.
San Jacinto Mts., botanical survey, Hall, 87.
Scientia, 81.
Scientific Literature, International Catalogue, 491.
Scripture, E. W., new machine for tracing speech curves, 447.
Sellards, E. H., Paleozoic cockroaches, 307, 489.

Setchell, W. A., Phycologia Boreali-Americana, 239.
Sherborn, C. D., Index Animalium, 334.
Sinclair, W. S., Mylagaulodon, new rodent from Oregon, 143.
Smith, E. F., Electro-Chemical Analysis, 80.
Smithsonian Institution, report, for 1902, Langley, 242.
Soudan, meteoric iron from, Cohen, 254.
Soufrière and Mt Pelée, report on eruptions, Anderson and Fleet, 489.
Sound waves and electromagnetics, Heavyside, 237.
Spectroscopic methods, Konen, 81.
Spectrum, solar, gaseous constitution of the H. and K. lines of the, Trowbridge, 243.
— ultra-violet of iron, Lehmann, 156.
Speech curves, new machine for tracing, Scripture, 447.
— from flames, 481.
Sperry, F. L., eruption of Colima, 488.
Stars in Northern Hemisphere, parallax of ten first magnitude, Elkin, 490.
Steel-works Material, Analysis of, Brearley and Ibbotson, 79.
Surface tension and viscosity, Burbank, 140.

T

Thomson, J. J., electricity carried by gaseous ion, 327.
Treadwell, F. P., Analytical Chemistry, 479.
Trowbridge, J., gaseous constitution of H. and K. lines of the solar spectrum, 243.
True, H. L., cause of the glacial period, 161.

U

United States Geol. Survey, bulletins, 196–199, 237; 200–204, 328; 22d annual report, 157, 328; Water Supply and Irrigation Papers, Nos. 58–74, 159.
Urals, geology and petrography, Duparc and Pearce, 83.

V

Vapor, diffusion of, into nucleated air, Barus, 472.
Vermont, report of State Geologist, 1901–1902, Perkins, 329.
Verrill, A. E., Bermuda Islands, 332, 333.

W

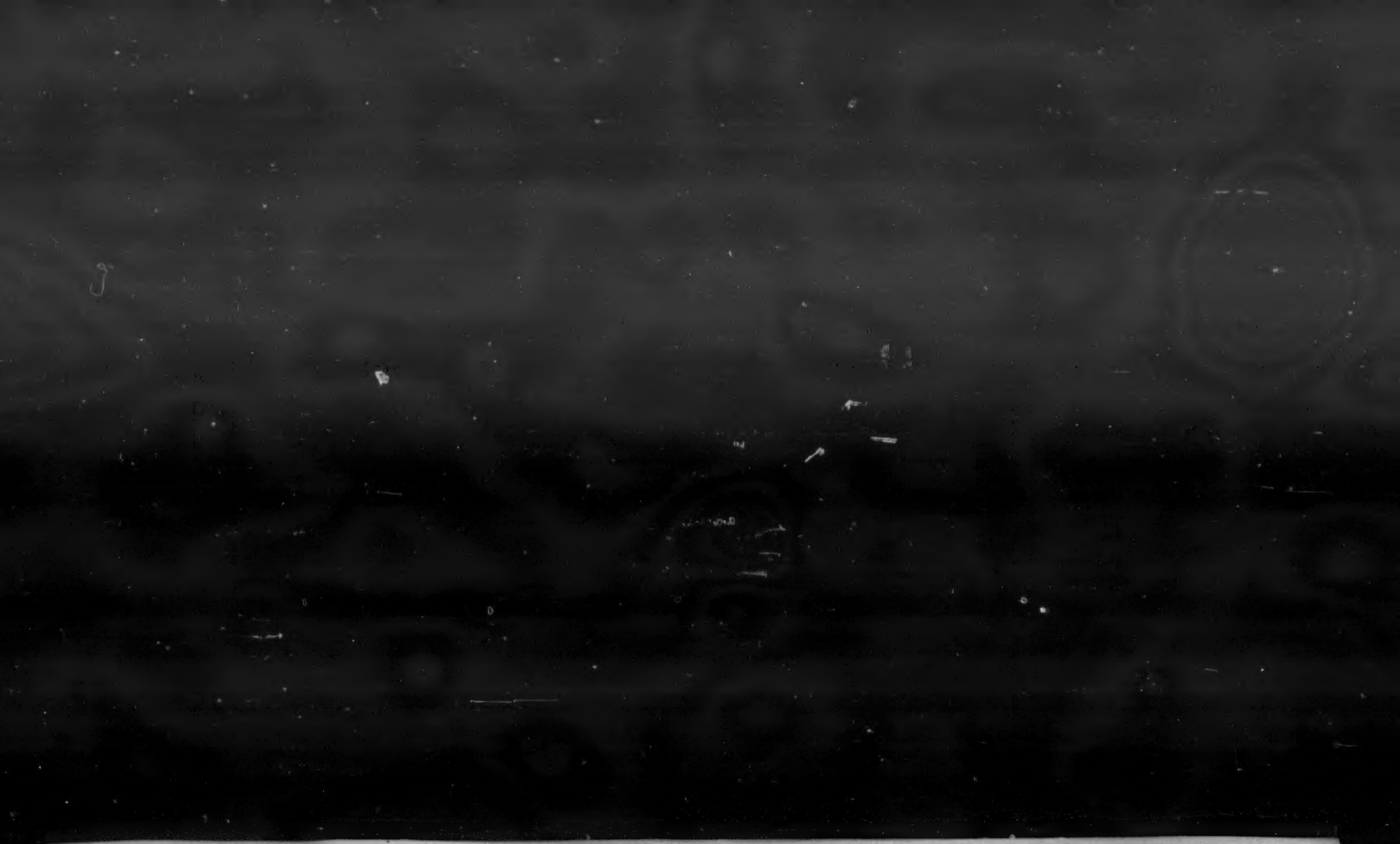
Walmsley, W. H., Photo-Micrography, 327.
Ward, H. A., Bath Furnace, Kentucky, meteorite, 316; Andover meteorite, 395.
Washington, Ellensburg Folio, Smith, 328.
— Monte Cristo, Ore Deposits of, Spurr, 159.
Water, purification by ozone, Van't Hoff, 236.
Weidman, S., amphibole hudsonite, 227.
West Hendon House Observatory, publications, Backhouse, 162.
West Indian Madreporarian Polyps, Duerden, 333.
Wieland, G. R., marine turtles, 211.
Wisconsin, lakes of southeastern, Fenneman, 238.
Wortman, J. L., Eocene Mammalia in Marsh Collection, 163, 399, 419.
Wright, A. W., obituary notice of O. N. Rood, 73.

X

X-Rays, velocity of, Blondlot, 236.

Z

Zirkel, basalts of the Rhine, 331.

ZOOLOGY.

Coral Reefs of the Tropical Pacific, Agassiz, 333.
Corals of Maldive and Laccadive Archipelagoes, Gardiner, 241, 489.
— West Indian, Duerden, 333.
Lepidoptera, North American, Dyar, 418.
Polyps, West Indian Madreporarian, Duerden, 333.
Veneridæ, synopsis of the family, Dall, 418.